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ST. THOMAS'S HOSPITAL.

CLINICAL LECTURE

DELIVERED BY

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THERE has been one death since I last had the pleasure of meeting you, Gentlemen. It was a case of phthisis, which was here before the commencement of the season. The man was not inspected, but there was nothing interesting in the case. Unfortunately we see cases of this kind every day. Nothing of course was done except to palliate the symptoms.

There was a case presented among the women of syphilis. I will not, however, speak of it. It was treated in the usual way.

INFLAMMATORY PILES.

There was likewise presented a case of inflammatory piles which occurred in a young man. The symptoms were a swelling around the anus, accompanied by great pain. It was presently cured by keeping him in the horizontal posture, the application of leeches, cold, and alum water. I think, that of all local applications in hæmorrhoids, nothing answers so well as alum. Of course when there is inflammation or great congestion, leeches are required: I think, as I have just said, that alum answers better than anything else, as a wash. Of course it is necessary to keep the bowels open, but it is very requisite to exercise care in not doing too much. If you give strong purgatives, you will make the matter ten times worse. Constipation ought to be avoided, because hard fæces necessarily irritate and cause great congestion of all the vessels, and therefore must increase the complaint. But, on the other hand, if you irritate the part too much by purging, you will have the bowels pressed down by the mere irritation; you will have the mucous membrane very much relaxed, and do as much mischief in this way as if there were constipation. Aperients are very proper, but purgatives are generally injurious, that is to say, the patient's motions should both pass through the rectum with as little effort as possible, and with as little irritation and relaxation of the parts as possible. Either extreme is most injurious. I have known many people have a fit of piles brought on by taking strong purgatives, and on the other hand you will see it occur every day by allowing the bowels to become costive. Of course, whether there be straining from diarrhœa or constipation, it matters not, the effect being the same.

EPILEPSY, WITH LARGE HEAD.

Trial of Iodine.

There was a case of epilepsy presented occurring in a boy. You will find that three-fourths of the cases of epilepsy that we have here occur in males; while St. Vitus's dance, on the contrary, occurs more frequently in females. I do not know how many cases of epilepsy I have at present under my care in boys.

Now this was a boy who had a very large head. When you see any irregularity in the shape or size of the head in epilepsy, the prognosis is usually unfavourable; at least much more unfavourable than if the head have the ordinary appearance. If it be connected with an overgrowth of a part of the head, or a deficient development, usually there is a strong predisposition to the disease; if there be organic disease present, the affection is not very easily treated. Now this boy had a head universally developed in excess: it was a very large head, and if it was so large from brain, and the brain was good, he would be a fine character. It is quite certain that he had a beautiful and powerful-shaped head, viewed in itself, but it was a great deal too large

fcr him, he being only fourteen years of age.

He had had the disease two years, and nothing could be noticed except the great size of the head and the epilepsy. It is possible that the great size of the head arose from more or less fluid within the head, or it might arise from hypertrophy of the bone; there might likewise be hypertrophy of the brain. I did not observe any deficiency in his intellect, but it was said that sometimes he was babyish. There is such a disease as hypertrophy of the brain, and it is mentioned by many writers. I never met but with one instance of it, and there the brain was good enough. The boy was remarkably clever, and was never contented with association with other children, but was anxious to be in the presence of adults, and would continually talk on abstract subjects. The result, however, of this activity of the brain was, that he was seized with hemiplegia and died apoplectic.

Now you will generally find, that cases of epilepsy that come into the hospital, yield to a certain extent, and yield not so much by the influence of a particular medicine, as by the patient being kept quiet, away from excitement; by being kept on low diet, cut off from meat and fermented liquors, by the bowels being kept regularly open, and blood occasionally taken from the head,—antiphlogistic measures being used to a certain extent. This boy had no fit after he came to the hospital. It was said that he had a fit once a month before he was admitted, but he was in the hospital from the 4th of October to last week, the 22nd of November, and during that time he had no fit at all.

Treatment.—The treatment consisted in shaving his head, putting him on fever diet, and keeping his bowels regularly open, until at last he had a little milk in addition to the fever diet. He was also bled once. This general sort of treatment is the utmost, commonly, that one can do; but having of late seen great use in many cases of cerebral disease from rubbing iodine into the head, I had it rubbed into this boy's head. If a remedy be good for one kind of chronic disease of the nervous system, it may be so for another, because such affections frequently result from the same internal state. As far as we know, what will produce one disease in one person, will produce a different disease in another; according, perhaps, to the situation and extent of the affection. We find very much the same means useful in many diseases of the nervous system, and in a number of them we frequently find no means useful at all. When a case of nervous disease, such as epilepsy, or paralysis, or insanity, is connected, as far as we know, with a morbid condition of the parts within, sometimes there is inflammation, sometimes effusion, sometimes congestion, sometimes various organic diseases, and accordingly, as one part of the brain be affected, we shall have different diseases, and according also to the degree of the affection. I gave him the chance of being benefited by iodine, on account of its great utility in cases of thickening and effusion. Iodine was rubbed in externally, and he likewise took the hydriodate of potash. He began with seven grains three times a day, and at last he took twenty-one grains about three times a day, with five drops of the saturated tincture of iodine. Whether it did him good I cannot say, but he was better before he went out, having had no fit while he was here, though he had been accustomed to have one every month for two years. His drowsiness went off, and his health we observed to be very good. Not noticing any paroxysm, I did not think it worth while to keep him in the hospital, but made him an out-patient.

QUARTAN AGUE.

There was a case of quartan ague presented, and of all descriptions of ague, the quartan type is the most difficult to cure. It occurred in a man who, at the commencement of the harvest, went into Kent, and he stated that he remained there till the beginning of the month. After he had resided there about a fortnight he was seized with ague. He first of all had rigors, and was very thirsty, and became warm, and he then said that he had a regular attack of ague every fourth day.

Treatment.—The treatment was very simple. I gave him five grains of sulphate of quinine every six hours, and I find he had but one paroxysm after his admission. You will find it of great importance in the treatment of this disease, not to give up the remedy immediately on the cessation of the complaint, because, just as in syphilis, it will return, unless the remedy be given for some time afterwards. If a person omit mercury as soon as a sore is healed, there is every probability that he will have a return of the disease in some shape or other, and so with regard to ague. I therefore make it a rule to continue the remedy for a fortnight or three weeks after the paroxysm has ceased. I gave him a stock of the medicine to take out with him.

DREADFUL CASE OF "TIC DOLOUREUX."

The disease to which I am particularly anxious to direct your attention on this occasion, was one of a most horrid description, and one in which I am sorry to say I did no good whatever, or, at least,

only produced a temporary alleviation from time to time. It was a case of neuralgia occurring in one of the fingers. You know that by *neuralgia* is meant a pain of the part, the word being derived from *neuron*, nerve, and *algos*, pain. I will read his case to you, for it is one particularly interesting, and it must become more so when the patient returns, for he is only gone out for a time.

This man was a journeyman printer, aged 32, and had been ill two years. He first of all had pain of the legs, arms, and wrists, and when he came in, the pain was confined to the middle-finger of the left-hand, on each side, along the course of the nerve. The other fingers of the same hand were benumbed,—were without any great degree of feeling, but the thumb was unaffected. Originally he had had pain on the right side of the face—that is on the opposite side of the body, and the pain then commenced in the submaxillary nerve, and extended upwards, so as partly to affect the second branch of the fifth pair, as well as the third. It is, therefore, to be recollected, that he had had neuralgia in another part of the body. All this, however, had ceased a month before admission, at which time he had only pain on each side of the middle-finger, and after that had existed some time, the *other* fingers had become benumbed.

The pain was of a very agonising character; a plunging, stabbing pain, as though you were running a penknife along the finger. Patients usually describe the pain in neuralgia as stabbing and plunging. The least touch gave him violent pain, like an electric shock. As the least touch produced such violent pain, he could not bear his nails to be cut, and the consequence was, that the nails of that hand had grown to a great length. He could not sustain the motion of the hand which the cutting of the nails necessarily produced, and the agony of the pain was such, that he bit the nails of the fingers of the other hand, so that the nails on the fingers of it were eaten down by him, in his agony, as far as they could be. The appearance was certainly very remarkable; for on one hand the nails were, as I have just said, as short as they could be; and on the other hand, though they were not so long as they might be, yet they were of very great length. I believe whenever he had a very violent attack of pain, the nails of the affected hand became discoloured, and remained so for some time.

I could detect no cause for this at all. I could connect it with no obvious morbid state, and, therefore, what produced the pain I could not tell. I could only say there was a pain, and that the pain was clearly situated in the nerves — following the course of nerves, affecting other parts also, exactly in the situation of branches of known nerves, and having the usual character of pain of the nerves, or at least, what it very frequently is, stabbing and plunging. There was no heat or inflammation of the fingers; nothing whatever to be seen, but yet there was agonizing pain, and the slightest touch aggravated the pain when present, or brought it on at a moment when he scarcely felt any.

Treatment.—One of the best remedies in this disease, but by no means a specific, and by no means so successful I think as in some other nervous complaints, is carbonate of iron. Its efficacy in this disease, so far as I know, was first pointed out by Mr. Hutchinson, a surgeon in the country, who, I believe, is now dead. Other people might have given the remedy a trial before, but it was just in the same way that they would have given copper, or zinc, as a thing that was sometimes useful; but this gentleman had tried many remedies in the affection, and at last he determined to find out what was most successful; and from observation he found that iron had great power over the disease. As this case was very severe, and had lasted two years, I at once gave the man a large dose of iron.

Now with respect to doses, I have said a hundred times over, and I am always anxious to repeat it, that nothing can be more absurd than to give a large dose of any medicine, if you are sure that a small one will answer, or even if you think so; but I deem it quite as absurd to give small doses when you know that they will not answer, or to give them when there is no probability of their answering. When a disease is very violent indeed, of course you must bring up your forces in a commensurate manner. No one would think of sending a regiment of soldiers to remove an old apple woman; but no one would send two policemen to storm a castle. Nothing can be more absurd, I think, than to make remedies disproportionate; either, on the one hand, to give large doses when not necessary, or, on the other hand, to give small ones when large doses are required. This man's complaint having lasted two years, and carbonate of iron being an innocent remedy, provided you keep the bowels regularly open, he took half an ounce three times a day; and when he had taken that for five days without any benefit whatever, he took the same quantity every four hours.

Now this did him a certain degree of good. He was better. Still he had pain sufficient to keep him awake at night; and I gave him, in addition, a quarter of a grain of muriate of morphia. The benefit was but temporary, and I applied to the finger a solution of the cyanuret of potassium, which has

been so much praised by the French. But it did not relieve him materially, and the solution was then made stronger than the French have recommended it. It was carried as far as twenty-four grains to an ounce of water. After a time it was suggested, that it was merely the cold which did him good, and I applied ether to see if that would relieve him, and it did so, much more than the solution of cyanuret of potassium had done. Still, however, he was very little better. The amendment which he at first experienced on taking the iron ceased, and I was obliged to increase the dose of medicine, and likewise the muriate of morphia, for he obtained no sleep. He took a whole grain of the latter every night. The iron was then increased to the quantity of an ounce, and it was given every four hours. His health improved under it, and from being pale and thin, he lost his great paleness, and gained flesh, and thought himself quite another man, so far as his general health was concerned. The quantity of cyanuret of potassium was increased now, to a drachm in an ounce of water; but it afforded no relief. He still found more benefit from ether. One could not expect a man to continue swallowing more than an ounce of iron every four hours. He would have taken it in any quantity, but it was necessary to consider his stomach, notwithstanding his good will to take the medicine. I therefore gave him another form of iron also, the sulphate. I do not know that the carbonate is superior to the sulphate in this disease, or in chorea, and the latter may frequently be taken the most easily. He began with five grains, in conjunction with the carbonate of iron, and took both every three hours. His agony was still extreme, and the sulphate was, therefore, increased to ten grains, and afterwards to fifteen. He received some degree of benefit, but it was only temporary.

I thought now that the iron had had a very fair trial, and it was only relinquished on the 26th of July, he having begun to take it on the 8th of March, so that he had continued it for three months. This was a very fair trial, both with respect to the quantity of the dose, and the long continuance of its exhibition.

I have very frequently failed with this remedy in neuralgia, and you must expect to fail with it, exactly as you must expect to fail with any other remedy in many nervous diseases, because the source of the affection is so very various. Suppose that, in this case, it arose from disorganisation in a part of the nervous system, all the iron you could give him would do him no good, and all the narcotics you could exhibit would only deaden the pain. It is just the same with epilepsy and paralysis. No one remedy can be adapted to all cases of the disease. Although we knew what the disease was, namely, neuralgia, yet the cause of the disease was not known. What was the local state of the nervous system giving rise to it, I could not tell, and the same thing occurs in epilepsy. If you can discover inflammation, you may employ the remedies for inflammation, but if you do not, you must then exhibit what has been known to do good in the disease, and take the chance of the remedy doing good or not. It is always possible that there is something or other over which the remedy can have no control.

I then relinquished the iron altogether, and endeavoured to apply strychnine to the finger. We attempted to blister the finger, and then sprinkled half a grain of the strychnine upon it. It was, however, with very great difficulty that we produced vesication; very imperfect vesication was effected, and therefore it was not very well managed. I still determined on trying whether the strychnine would do him good or not, and I therefore exhibited the 12th of a grain internally. As strychnine is so powerful an agent, I never like to begin with a larger quantity than that. It was then increased gradually to the eleventh, tenth, ninth, eighth, and so on, of a grain. Still he was no better, and I was obliged to increase the muriate of morphia to two grains every night, one grain being said to be equal to four grains of opium. The man told me with tears in his eyes, that the agony was such that he never slept.

The strychnine was increased now to half a grain three times a day, and I had the finger smeared with croton oil. He could not bear to have it rubbed in, and therefore it was gently smeared on the part, but produced no great irritation. His health now began to decline. He was taking on the 7th of August three-fifths of a grain of strychnine three times a day, and two grains and a half of muriate of morphia, but he began to take the iron again. I gave it him without any hopes of its producing benefit, but for the purpose of improving his general health. He began with half an ounce three times a day, which was increased to an ounce, and then to four times a day. His health soon improved again, but the pain continued unabated, and the muriate of morphia was increased to three grains twice a day. His pain was such, that I was obliged to give him an opiate in the day time as well as at night. The strychnine was not increased beyond three-fifths of a grain three times a day, and as it did him no good, merely produced some twitching of the limbs, it was relinquished on the 21st of August, never having been of the slightest service. Arsenic has been

known to be serviceable in some cases combined with an alkali as in the liquor arsenicalis. He commenced with this on the 21st August, and began with small doses three minims three times a day, not taking it on an empty stomach. This was increased gradually to nine minims, but without any effect on the disease, though it began to make him sick. Nine minims I consider a very full dose. It, however, disturbed his stomach, and to prevent the sickness he took two minims of hydrocyanic acid three times a day, half an hour before the arsenic. This did not lessen the sickness, and I therefore was obliged to increase it to three minims, and went gradually on till I arrived at a dose of nine minims three times a day. This entirely prevented the sickness arising from the arsenic. Hydrocyanic acid has the very valuable property of stopping vomiting, provided this does not depend on inflammation. Knowing the power that it has of tranquillizing the stomach generally, though it does not invariably produce relief in gastrodynia, I thought it would stop vomiting. I never heard that it had been used in such cases, but I thought it must do good. After employing it for two or three years, I thought it might prevent medicines from causing further vomiting. I tried it, and I found that it had that power in a very marked degree. Finding in this case that two minims had no effect, I gradually increased it, as I have before mentioned, to nine minims, and it hen entirely stopped the sickness caused by the arsenic.

All this time the arsenic did no good, he bore it very well, but it was of no service to him, and I therefore gradually increased it to the largest dose that I ever gave—viz. twenty minims three times a day. He now began to look thin again—did not look so well as before. I could not, however, tell whether it was the result of the arsenic. It might merely have arisen from his extreme suffering, but still it was right that I should discontinue the medicine, and I did so without his having derived the slightest benefit from it during the whole period of its exhibition. As it was necessary to procure sleep, the muriate of morphia was gradually increased up to six, and at last to eight, grains twice a day. His agony was such, that he begged to have the opiate, and he also begged to have it increased, otherwise he said he could scarcely exist.

He was taking, therefore, at last, eight grains of muriate of morphia, twice a day, which gave him ease (I got him to omit it once or twice, but he suffered so much, that he begged to have it again) and twenty minims of liquor arsenicalis, always taking before it nine minims of hydrocyanic acid. This he bore perfectly well, with the exception, that he looked as ill as he did when he first came in, not worse, but just as he did before I gave him the iron.

He now wished to go out of the hospital for a fortnight for a change, and he was supplied with a quantity of muriate of morphia to take with him till he came again.

I attempted, during the time, other local measures. He once rubbed the extract of stramonium on his finger frequently every day, and it relieved him for a time. He rubbed also the extract of belladonna, two or three times a day, and this, he said, certainly produced relief for some time; but I am sorry to say, that at the very last he was nearly as bad as at first. He himself maintained, that he was a great deal better; he did not *allow* it, but *maintained* it. I was afraid that he was not improved, but he assured me that he was better after he took the iron; that altogether his sufferings were not such as they were before he came to the hospital; but still they were dreadful. He appeared to be an excellent man, a man of a strong mind, but in his agony the tears were seen running down his cheeks.

He will return, but I have no idea of medicine doing him any further good. We have given him the most powerful remedies, and these have been used in the most powerful manner, although with great care; but he is not materially better, and, therefore, when he comes back it is to be considered whether or not he shall have his finger amputated. I have very little hope, however, in the operation, and for this reason—cases have occurred in which the operation has failed, the disease having re-appeared in the corresponding nerves of the other hand, and in the trunk of the nerves the branches of which have suffered amputation. Still it may be right to make the trial. I have little hope of success, however, for another reason, because he has had the affection before in the nerves of the face. It looks to me as though there were some cause of irritation high up in the nervous system—in one of the centres of the nervous system. I imagine that in some part of the brain or spinal marrow there is a source of irritation, and, therefore, that amputation will only relieve him temporarily, that the disease will re-appear either in the nerves of the same hand, or of the other hand, or in some other part of the body.

I may mention, that the operation has been performed with success. Mr. Wardrop cured a case by amputation of the finger, the particulars of which are mentioned in the eighth vol. of the *Medico Chirurgical Transactions*. Another successful case is mentioned by him in the fourth vol. of the same work, and I may state, that in the *Philosophical Transactions*, Sir Everard Home remarked, that mere division of the nerve

proved fatal. You might think that dividing a nerve would be sufficient, but the reverse has proved to be the case, for the nerve will unite again, and Sir Everard Home mentions, that in his case great irritation of the nervous system ensued, and the patient died.

Not to make any other remark, I am anxious to say that I conceive I should not have done my duty to this man, if I had not pushed the remedies in the way I have stated. I never, as I have said before, would use large doses where small ones are sufficient, and it is to be recollected that in this patient I began with the arsenic, the opiate, and the prussic acid, in ordinary doses; but as these produced no injurious effects on the constitution, and no benefit was effected on the diseased part, I of course carried them farther. If I had stopped at two grains of the muriate of morphia, or two drops of the liquor arsenicalis, it would have been bad practice. If I had begun with ten drops of the liquor arsenicalis; or a large dose of the prussic acid, that would have been bad practice; or if I had increased the remedy beyond two drops, provided they had done good, that would have been bad practice; but I conceive that nothing is so absurd as to prescribe one particular dose, and say that a person shall not take more. Three grains of sulphate of quinine, or five drops of liquor arsenicalis, may be considered a fair dose, and five minims of prussic acid; but when a disease will not give way, and yet a remedy does not disagree, it is an old woman's practice, and not creditable to any man of sense, to go on with the same dose. If a disease be very violent at first, and you are anxious to spare the patient as much suffering as possible; if you see that if you do not give a remedy quickly, the patient's life will be forfeited, then you will be justified in giving a large dose at first, for there is no time to go on and reach large doses. Suppose it were a case of ague, shattering the constitution of an old person very much, and that every paroxysm was likely to do serious harm. I should immediately give a large dose of sulphate of quinine to prevent, if possible, the occurrence of another paroxysm. This is just what we are obliged to do with regard to venesection. We do not say that for a certain complaint we should invariably bleed to eight, twelve, or sixteen ounces; for sometimes we do not abstract more than four ounces, and in a different state of the system and of the constitution generally, we take forty ounces. We do not say, respecting purgatives, that a patient should always take a fixed quantity in order to open the bowels, but we prescribe one dose for one person, and another for another. All persons allow this with respect to certain medicines, but they forget it with respect to others.

To show how necessary it is to bring up your force according to the constitution, I may state that you will meet with patients every day who will be purged with the fifteenth of a drop of croton oil, and yet there is a man now in the hospital who is taking five drops most days. It is a case of colic arising, I fear, from an organic obstruction in the alimentary canal, and he regularly takes a drop of croton oil every morning, and when that is not sufficient he takes another two hours afterwards, and frequently goes on to the fifth drop. The force is adjusted to the degree of the resistance. It would be madness in me to order that man five drops of croton oil at a time, or to have ordered him even one drop before I knew the exact state of his inside. He would never have a motion if I gave him a quarter of a drop only, or if I contented myself with merely giving him two or three drachms of magnesia. I know that when you are asked questions at your examination, it may be said that you will kill a patient if you give a large dose; and so you might, if you gave it him at once; but just as a surgeon sometimes merely makes a puncture, and in other cases makes a free incision two or three inches in length, and sometimes two or three incisions, so you must act in the practice of medicine. It would be quite as absurd for an examiner at the College of Surgeons to say that you should always make an incision exactly a quarter of an inch in length, as it would be to lay down any rule for a dose of medicine. No good surgeon would make an incision three inches in length where one would do, but every good surgeon would condemn another who merely made a puncture when a long incision was required.

Respecting this particular disease, *neuralgia*, I may mention, that it does not always appear, as in this man. This is what was called, formerly, *tic douloureux*. When a chronic pain is situated in the course of particular nerves, and comes on violent stabbing, plunging, the pain being increased by the slightest brush, it was termed "tic douloureux;" but neuralgia, although it is synonymous with it, is used to signify any pain in a nerve, and many such cases may be easily cured. Several of these instances are nothing more than rheumatism affecting a nerve: you see it every day in the sciatic nerve. Rheumatism affects ligaments, tendons, aponeuroses, (and perhaps muscles), chiefly, but sometimes it affects a nerve, and sometimes the neurilema covering the nerve, and great pain is the consequence. In that case it usually occurs from cold, and the pain is generally of an aching character, and periodical or con-

stant; it is not a violent stabbing pain, or if it be, there is a constant aching also, and it is not increased by the least brush, though, of course, it is by motion.

Such an affection is sometimes cured with great ease. You treat it exactly as you would any other description of rheumatism. If it be inflammatory, or if heat make the part worse, you must keep the patient cool, apply leeches, &c., and give colchicum and mercury, and it will go away. If there be nothing of the kind, or if heat make it better, you must give stimulants, ammoniated tincture of guaicum, preparations of iron, adopt acupuncture, and the exhibition of mercury will do good. You must treat the case just on the same principle as common rheumatism, but that is a different affection to what this man labours under. Sometimes you have rheumatism in the face, the pain running along particular nerves, coming on after the clothes of the individual have got wet through, and the eyes will run and be suffused. Leeches, the exhibition of colchicum, and especially mercury, will here be useful.

All these classes together are called "neuralgia," and persons will tell you that they have had hundreds of cases of neuralgia, and have cured them all, and the cases are set down as cases of "tic douloureux," though they are very distinct from that affection. Rheumatic neuralgia is a very common thing. They are cases of which we have always plenty to treat, and generally they are not more obstinate than other rheumatism, and are cured as frequently as rheumatism any where else. But such an instance as I have now alluded to, is a rare occurrence. I do not see a case of old-fashioned tic douloureux above once in a year. I know it is an obstinate disease, a disease which the carbonate of iron relieves very much, but in a great number of cases iron will not cure it: it relieves it for a time, as it did this man, but too frequently no more.

It is a chronic disease, and the reason why you frequently cannot cure it is, that it is often connected with some organic affection. I know that, after death, nothing has been found; just as after epilepsy and paralysis. Sir Charles Bell and Magendie both state that they have examined nerves which have suffered violent pain, and have found nothing. On other occasions, however, something has been found. A little tumour has been discovered in the nerve; disorganisation has been found, or at least the veins all around the nerves, and the neurilema, have been varicose, and a collection of fluid has been met with on the nerve under the neurilema; and sometimes the stumps of decayed teeth have produced it. In the latter case the presence of the stumps may keep up the pain, and by removing them you cure the disease. Sometimes a piece of diseased bone has been found; sometimes there is disease of the base of the skull around the foramen; an overgrowth of the part; so that the nerves have been pressed upon in their exit from the cranium. Sir Henry Halford read an essay before the College of Physicians, in which he stated that he had seen cases, and other cases of tic douloureux were mentioned, in which, after death, the bones were found diseased. Now if there be irritation of that description in this man, and I cannot say that there is not, how can carbonate of iron or arsenic do him good? Sometimes common neuralgia appears connected with malaria, and then quinine may do good; if it put on an intermittent form, quinine is sure to be serviceable. As to old-fashioned neuralgia, such as occurred in this man, and such as we see every now and then in the nerves of the face, it is a disease which you may occasionally cure by iron and arsenic; but so far as my experience goes, in the greater number of cases the relief is only temporary. You may certainly do great good, but the relief is often temporary, and the disease frequently either has worn itself out at last, or has continued more or less till the patient died.

FRACTURE
OF THE
NECK OF THE FEMUR,
TREATED ON DUPUYTREN'S PLAN.

By W. C. Clough, *Esq., M.R.C.S.L., Tonbridge Wells.*

Mrs. H., eighty-five years of age, of diminutive figure, rather thin, and of a good constitution, having seated herself upon a high stool near a black-currant bush, for the purpose of gathering its fruit, suddenly fell down, and hit upon the trochanter-major of the right femur. She attempted to raise herself from the ground, but was unable to do so; and her husband, who happened to be in the garden at the time the accident occurred, took her up and carried her into the house.

This was on the afternoon of the 26th of last July. On the 27th I saw her, and found the usual appearances of fracture of the neck of the femur presenting themselves. In this case the injured limb was two inches shorter than the sound one, it was also much swollen, and considerably discoloured in the neighbourhood of the

trochanter. The fracture I believe was a transverse one, and situated within the capsular ligament. The patient complained of severe pain, which was greatly augmented on attempting rotation of the limb; the crepitus was distinctly heard, and on applying slight extension, the natural length of the member was restored. Now, not having a proper fracture apparatus, I, in this case, applied two splints, and placed the limb on a double inclined plane.

Matters appeared to be going on extremely well for the space of nine days, but at the expiration of that time the pressure of the splints had produced three large sores; two on the outer, and one on the inner, side of the limb. I was, consequently, obliged to remove them, and, fortunately, just at this period, some observations of the Baron Dupuytren, on the "treatment of fractures of the neck of the thigh," &c., were thrown in my way. The plan proposed appeared so feasible and so simple, that I at once determined to follow up the treatment recommended by him.

The apparatus which he describes is thus made. A double pillow, made round like a bolster, and kept in that situation by ribbon, is placed at the top of a double inclined plane, made by several pillows placed one above the other, and stitched together. One of these inclined planes extends from the bend of the knee to the tuberosity of the ischium, the other to the heel—the junction, or apex of the planes, corresponding to the bend of the knee. The leg is fixed by passing a sheet, folded like a cravat, over it, and fastening each end to the sides of the bed. Another may be placed in like manner over the middle of the thigh, to keep the patient more effectually in that situation. During the first month the thigh is moved every, or almost every, day, by drawing down the lower fragment by gentle traction, in order that it may be in perfect adaptation with the superior. When the consolidation is supposed to be effected, the inclined plane is gradually lowered by taking away one of the pillows, until the whole are removed. The patient should keep his bed afterwards for some days, without once leaving it, and when he does it should be with great precaution. Now this really simple treatment is available in all cases, and the great facility with which the fracture bed (so we must term it) is constructed, renders it a great desideratum in country practice.

On this bed, then, I placed my patient, and certainly the case went on remarkably well. She at first complained of pain, particularly about the knee-joint, owing to the unnatural position of the limb. This however soon went away, or was forgotten; but was succeeded by a dropsical state of the entire body, so that I really imagined I should lose my patient from this affection alone. However, that was not the case. Blue pill was now administered every night, in combination with squills, digitalis, and opium, and the supertartrate of potass mixture during the day.

Under the administration of these medicines she improved, the anasarcous state of the body gradually disappeared, and she speedily recovered. This was about the sixth week. On the 20th of September the patient could raise her foot in a very slight degree, and by bringing the limbs into opposition, the two patellæ were found to be on the same level. On the 2nd of October, and succeeding days, the pillows were gradually abstracted, and the movements of the limb were now performed with a great degree of facility. On the 13th, when I saw her, she expressed a desire to get up. In this I indulged her, and she was now daily placed on a high arm-chair. From this time also she began to exercise the limb, but was not allowed to bear the least weight upon it. On the 4th of November (the present month) she could walk with crutches, and is at this time quite well. There is now nothing particularly discernible, except perhaps a very slight degree of shortening; there is not the least eversion of the limb.

Remarks.

From the history of the foregoing case, it is quite evident that we may now treat fracture of the neck of the femur in a much more simple manner than has been done heretofore. Messrs. Earle and Amesbury's fracture beds are excellent, but cannot always be procured; but here is a contrivance which, at any time and in all places, can readily be had; for what can be conceived more simple than a few pillows, and two or three long folds of linen doubled like cravats. The proceeding too, as the Baron Dupuytren justly observes, is based on anatomical and physiological knowledge. Now it may be asked, *in this case*, whether osseous union had taken place or not? For my own part I should be disposed to answer in the negative, and on this account simply—viz. the advanced age of the patient, but this possibly may not be an objection. With regard to bony union in these cases, there has been much controversy; the French surgeons generally affirming that it can and does take place, while almost all our own eminent English surgeons advocate the contrary opinion. Sir Astley Cooper, I believe, is amongst the number; but Mr. Lawrence (whose pupil I feel proud to have been) is of opinion that osseous union

can take place, and in speaking of it in his lectures, he says, "in order to prove that fracture of the neck of the thigh-bone can unite, it would be necessary to show us specimens of fractures that have united, because, if one can be shown, there is an end of the dispute on the subject. I believe that such fact can be shown."

Tonbridge Wells, Nov. 22, 1832.

On the Influence of Physical Agents on Life. By W. F. Edwards, M.D., F.R.S., &c. Translated from the French. By Dr. Hodgkin and Dr. Fisher, with an Appendix and Notes. London, Highley, 1832. 8vo, pp. 488.

This is a most valuable work in every point of view. Of the merits of the original it would be superfluous for us to speak in lengthened terms, especially since the very copious analysis we propose to make will enable our readers to form a just conception of the great and peculiar talents, the experimental nicety, the indefatigable patience, and the singular tact for exact induction, which are united in the gifted author, Dr. Edwards. Of the qualifications of the present version, however, this is the appropriate place to speak, and we are happy to declare our conviction, that its execution combines every excellence of which a translation is susceptible. All the precision of the original is preserved, while every redundancy of expression is skilfully and cautiously pruned away. In the appendix and notes there is also much matter of considerable value, sufficient indeed to induce us to pardon Dr. Hodgkin for an error of judgment for which we should otherwise have called him to a severe account. We allude to the unaccountable fact of his having permitted Dr. Stevens to meddle with the appendix, and mix up his nonsense with the noble conceptions, and sage inductions of Edwards and Spallanzani. The introduction of Dr. Stevens's chimeras forms the only blemish in the work.

Dr. Edwards's portion of the book occupies 285 pages, which are divided into four principal parts. Of these, the first relates to the effect of external agents on the *Batrachian reptiles*; the second on *fishes* and *reptiles*; the third on *warm-blooded animals*; and the fourth on *man* and *vertebrated animals*. We should add, that throughout the volume Dr. Edwards's researches apply to the *air* in its several conditions of quantity, motion or rest, density or rarity; to *water*, in a liquid state, and in a state of vapour; to *temperature*, as modified both in degree and duration; to *light*; and to *electricity*.

The first part is subdivided into six chapters. Of these, the first treats of *asphyxia*, and contains details of several interesting experiments on the respiration of Batrachian reptiles.

To trace the series and order of the actions which supervene during the extinction of life by asphyxia, is a problem to which the master minds of Goodwin, Bichat, and Legallois, were successively and not unsuccessfully directed. But the experiments of these physiologists were almost exclusively performed on animals with *warm* blood, while the phenomena presented in those with *cold* blood were passed over with injudicious neglect. This topic Dr. Edwards fully investigates, previously inquiring "whether the media in which asphyxia may take place, have not a particular influence independent of that exerted on the lungs." Of these media, the most important are air and water. It would be difficult to imagine anything more masterly than the experiments by which Dr. Edwards seeks to decide this question.

Analyzing the separate and leading systems which compose the framework in which life displays its agencies, Dr. Edwards, by cutting out the heart and bulb of the aorta, and thus annihilating the circulation in animals which live several hours after this operation, studies the comparative action of air and water on the *nervous* and *muscular* systems alone. Four salamanders of the species *triton*, were thus treated. Two were placed in water, and two exposed to the air, *at the same temperature*. The latter lived double the period of the former. The same result was obtained on an extensive repetition of the experiment. The inference is consequently inevitable, that "*air* has a superior vivifying influence on the system of these animals independently of its action by means culation and respiration." Varying the experiment, it was sought to ascertain

whether the circulation of venous blood deprived of the influence of the air, contributed to sustain the action of the nervous and muscular system. An equal number of frogs whose hearts had been removed, and of others left entire, were placed in water deprived of air. The result in all cases exhibited a marked difference, sometimes twenty hours in favour of the latter. Hence the "circulation even of venous blood is favourable to the action of the nervous and muscular systems, though incapable of maintaining life beyond a very limited period."

Strangulation next engages the Doctor's attention. Water having been shown by the first experiment to exert a positively deleterious influence on the nervous system, it was desirable to ascertain whether in a less noxious medium the circulation of the blood *though unaërated in the lungs*, might not prolong life still further. To elucidate this question, six frogs were strangled with pack thread, and left exposed to the air. At first they were paralysed, but afterwards they recovered to a great degree, and lived from one to five days. A similar number died in water in ten or twelve hours. This marked difference in the duration of life, proves distinctly and indisputably, *either* that these animals can live for many days without any other action of the *air* than that exerted on the nervous system, *or* that the blood is acted on by the air through *the skin*. That such cutaneous respiration does occur was proved by the next experiment, in which carbonic acid was detected in the air with which strangled frogs were placed in contact. Additional experiments with frogs, toads, and salamanders, exposed to the air, encased in plaster or sand, or placed under an exhausted receiver, showed further, that, independently of the action of the air, the degree of perspiration influenced the duration of life. Thus, where air had access to the skin, and perspiration was impeded by immersion in sand, life was prolonged for hours longer than by exposure to the open air, which led to the rapid desiccation of the animals operated on.

But the preceding facts may be modified by various circumstances, some of which are well susceptible of experimental investigation. *Temperature* is, perhaps, the most remarkable, and to this Dr. Edwards devotes his second chapter.

By repeated experiments the curious fact was thus ascertained, that as the temperature of water is reduced, the duration of the life of frogs is increased, until, at 32° Fahr., it becomes absolutely tripled. On the other hand, elevation produces a corresponding abbreviation of life, until, at 138° Fahr. death was almost immediate. But before a direct reference could be made from these experiments, many modifying circumstances remained to be examined. For example, the influence of climates and seasons, the mode of life of animals, and the effect of habit. With respect to the agency of the season, some most remarkable events were observed.

In the month of July, frogs immersed in aërated water at 60° Fahr. lived from one hour to two hours and 27 minutes. In November, at the same temperature, the duration of life was doubled. All other circumstances having been the same in the experiment, the season alone must have been the cause of the difference. How does the season operate? By pressure of the atmosphere? No. The difference of pressure during these experiments was too inconsiderable to be noticed. By light or electricity? These must be left out of consideration until we are supplied with more exact means of appreciating the degree of their intensity. In short, the investigation of this branch of the subject, as well by induction as by experiment, led to the double inference, that the temperature of the water at the time of immersion, and the temperature of the air for some days previous to immersion, exercised a marked effect on the duration of life, and contributed to its prolongation in proportion to the descent of the thermometer from 60° to 32°, at which latter point the limit of the observation was fixed.

The following chapter treats of "the influence of the air contained in water." To be amphibious, means, in the strict sense of the word, that an animal can respire, both in the atmosphere and by means of the air contained in water. The Proteus and Siren were alone supposed to enjoy this faculty. But in all these animals, as well as in the axolotl, the larva of the salaman-

der, and the tadpole, there exists the double organisation of lungs and gills. The adult batrachian, on the other hand, is furnished with lungs alone, and might, therefore, be presumed to be incapable of respiration in aërated water. To investigate this point, frogs were placed in equal but small quantities of exhausted and aërated water, at the same temperature; the former lived from three to five, the latter from six to ten hours. Aërated water, therefore, exerts a decided influence on the prolongation of the life of frogs. Does it support life indefinitely?—is the next question Dr. Edwards examines, and the experiment by which he solves the problem is so beautiful, and its physiological results are so curious and important, that we extract the author's own description, as translated in the version before us.

"Although the want of organs specially constituted to act in the air contained in water, rendered it improbable that frogs could live in water like fishes, I thought I ought to leave nothing untried, in order to determine the influence of aërated water upon their existence in that liquid.

"On the 4th December, the temperature of the room being 43° Fahr., a male frog of the species *rana temporaria*, was secured at the bottom of a glass vessel holding 17½ pints of Arcueil water, by means of a transverse wire grating. The next day the water was drawn off with a syphon, till only a sufficient quantity was left to keep the animal covered, when the vessel was replenished with fresh water. This was repeated daily. The frog not merely survived for eight days, the longest period for which Spallanzani had been able to keep a frog alive in water at 1°, or 0·5, but continued to live to the 25th of February, that is more than two months and a half, during which period the temperature had varied from 32° to 51° Fahr. An accidental neglect to renew the water occasioned the death of the animal.

"This experiment shows the remarkable fact, that frogs are really amphibious, since they can not only breathe the air of the atmosphere, but can also live exclusively by means of the air contained in water."

Through what organ then, is the vivifying influence of the air exerted upon these animals? Inspiration in frogs is performed by deglutition, in which process the movements of the fauces are distinctly perceptible. Now these movements cease altogether in water. Moreover, the lungs, after such immersion, are not filled with water; consequently respiration is not performed under water, either by deglutition or by contact of the water and lungs; therefore the action of the aërated water is on the skin alone. What changes are thus effected in the air, are examined in another part of the work, the author here confining himself to the observation, that the arteries in the webs of the feet during immersion in aërated water, are distinctly seen to contain florid blood.

Further experiments fully proved the interesting fact, that frogs are capable of this double respiration at all *seasons* of the year, But if the *temperature* of the water be increased, the death speedily takes place. The difference between 47° and 53° is even sufficient to abbreviate, remarkably, the duration of life. Whether from an incapacity of cutaneous respiration, or from a positively prejudicial tendency of the increased temperature, the experiments in this section do not determine. Increase in exposure to the air, viz. by frequency of renewal, or occasional ascent to the surface, of the water, enables frogs to live in an increased temperature for a much longer period.

The next experiments related are still more curious and decisive. They completely establish the fact, that pulmonary respiration is totally insufficient to support the lives of frogs, or the other members of the batrachian family. Cutaneous respiration, on the other hand, is so powerful, *that, after excision of the lungs*, frogs will live on wet sand for several weeks.

We pass over the fifth chapter on the influence of the atmosphere on perspiration, inasmuch as the facts it contains are nothing more than experimental proofs of opinions already entertained by all thinking and rational persons. It is not, however, one of the least characteristic and valuable features in Dr. Edwards's labours, that with exemplary patience he ever seeks to establish by experiment, those doctrines which common sense alone would hold to be incontrovertible and beyond the necessity of investigation.

We now arrive at the sixth and concluding chapter of the first part. Absorption and perspiration are here considered, with regard to the effect produced on the weight of the body by contact of water with its external surface. From this inquiry it results, that an actual absorption of water

takes place, frogs gaining weight in proportion as the fluid of immersion is diminished. This absorption is more remarkable, as might be expected, after previous perspiration. It does not, however, stop at the point where the perspiration commenced but may proceed much further. Its amount also varies according to the temperature of the fluid. It is greater at first, and then diminishes by progressive decrements. The absorption of a given amount of water is effected in a shorter period of time than is its loss by perspiration. To revert to the influence of temperature, it is shown by Dr. Edwards's experiment, that absorption predominates at 32° Fahr., while at 86° Fahr., the losses by transudation are greater. Elevation of temperature during immersion has, moreover, a marked tendency to augment the excretions in animals of the class subjected to experiment. Here ends the first part of the volume.

In the *second part* the same method of investigation is applied to the influence of physical agents on *fishes and reptiles*. It comprises three chapters, first on *tadpoles*, second on *fishes*, and third on *lizards*, *serpents*, and *tortoises*.

The action of physical agents on the transformation of the *tadpole*, is one of the most curious questions in physiology. The influence of aliment is exemplified, by the change of form being hastened by a good supply of food. Temperature also exerts a paramount power. Many tadpoles, viz., those produced late in summer, are not changed in our climate the same year, owing to the lowness of the temperature. Atmospheric, compared with aquatic, respiration, is, again, materially concerned. In the course of his experiments on the adult frog, Dr. Edwards perceived, that during aquatic respiration these animals were in a very torpid condition. He thence inferred, that limiting the tadpole to aquatic respiration, would probably tend to continue its original form. To subject this to experiment, several tadpoles were placed in a perforated box, sunk in the Seine, and others in a vessel in which they could rise to the surface and breathe; the latter changed in due time, the former retained their tadpole shape until they had doubled or trebled in weight. It is worthy of remark, too, that the experiment was commenced at that age of the animal when transformation is just on the eve of accomplishment. Such are the most prominent points in the first chapter. In the second, *fishes* constitute the object of study. These the author, on the faith of experiments analogous to those on the frogs already mentioned, conceives to be similarly affected with the batrachiæ as regards the influence of aërated water and *temperature*. The fifth section of this chapter is replete with interest. In it the author examines the phenomena connected with the existence of fishes in the atmosphere. A chub and a gudgeon were wiped, weighed, and exposed to the air; when dead they were stiff and dry, and had lost from a fourteenth to a fifteenth of their weight, a loss which several experiments showed to be more than sufficient to cause death. Again, according to the author, we find other and powerful agents operating against the duration of life in fishes removed from water to the atmosphere; *the usual elevation of temperature*, for example; and, also, the cessation of the action of the muscles which move the gills. To be brief. From all his observations, Dr. Edwards infers, that the loss of water by perspiration and evaporation, is the cause of the death of fishes in atmospheric air.

We have placed the allusion to *temperature* in *italics*, in order to direct our readers to a very interesting note on this subject by Dr. Hodgkin, in which he adduces some curious facts indicative of the power of some fishes to sustain a temperature much higher than that which seemed to set the limit to existence in Dr. Edwards's experiments. Thus rotifera, eels, and infusoria, live in the hot springs of Savoy. Lonnerat saw fish alive at Manilla in a hot spring in which he could not bear his hand. Bruce at the ancient Thala found gudgeons in a spring which seemed almost hot enough to boil them. The chromis of Cuvier was found in the hot springs of Barbara, and the French consul at Tunis sent two fresh-water turtles to Cuvier from the waters of Urtica, which has the temperature of 113° Fahr. How far that mysterious and wonder-working agent termed "habit" may have operated in these instances, we cannot stop to inquire. They prove at any rate that

elevation of temperature is not the sole cause of death in fishes removed from the water to the air.

The last chapter in the second part relates to lizards, serpents, and tortoises. On these the several physical agents already alluded to operated in precisely the same manner as on the batrachiæ,—the modifications of effect produced, being, in all cases, distinctly referable to difference of texture, thickness, or porosity of the skin, or variety in the magnitude of the lungs.

The third part contains seven chapters, in which the chief relations of warm-blooded animals to physical agents are elaborately investigated. From an extensive series of experiments, Dr. Edwards first concludes "that the power of producing heat in warm-blooded animals is at its minimum at birth, and increases successively until adult age. The phenomena of hibernation next engages his attention. During this remarkable period the animals appear to be converted into cold-blooded creatures, their temperature is scarcely above that of the surrounding atmosphere, they are torpid, they take no nourishment, and their respiratory movements are irregular, feeble, and at long intervals. It is their temperature that Dr. Edwards principally studies. During summer, spring, and autumn, the hibernating mammalia are as warm as all other animals of the same order. As winter approaches, the experiments of Saissy denote a gradual diminution of heat. The same author has distinctly shown that these animals may, in any season, be rendered torpid by excessive artificial cold and diminished respiration. Dr. Edwards's experiments finally show beyond dispute that they possess an inferior power of *generating* heat—a condition in which they resemble the *young* of warm-blooded animals. In these, however, the condition is but transitory—it is permanent in the hibernating adult. But age is not the only cause by which the production of heat is modified. The effect of the seasons must, of course, be important. Every precaution being taken which the ingenious author could devise, numerous experiments were instituted on this subject, and the indisputable conclusion was established, that "the influence of the seasons effects a considerable change in the constitutions of warm-blooded animals; that continued elevation of temperature diminishes the power of producing heat; and that the opposite state of the atmosphere increases it, provided the cold be not too severe."

Asphyxia is next considered and investigated with regard to the duration of life in warm-blooded animals deprived of air. Buffon's celebrated experiment is first detailed, in which a large parturient greyhound bitch being compelled to bring forth her offspring under warm water, the puppies lived a considerable time. Legallois made a similar observation on rabbits, and he extended his experiments so far, as to gather the result, that this capacity of asphyxiated life diminishes as the adolescence of the animal proceeds. Dr. Edwards, pursuing these researches, found that *some* animals, such as the guinea-pig, from the very commencement of life enjoy but a very limited degree of this privilege. Investigating the cause of this difference, he soon arrived at the interesting conclusion, that those animals which, in the immature state, possess the highest faculty of generating heat, are those least capable of sustaining the privation of air, and *vice versâ*. Again, the influence of external temperature on the duration of life during asphyxia, is to be especially considered. This circumstance has a powerful effect, as we have already noticed, on the cold-blooded vertebrata. To examine its agency on the warm-blooded animal, kittens, a day or two old, were placed in water at different temperatures. At 32° Fahr. they lived four minutes and a half, at 50° ten minutes and a half, at 68° thirty-eight minutes and forty-five seconds. At 86° a retrograde course commenced, and at 104° they lived but ten minutes and twenty-seven seconds. Thus a cool temperature was the most favourable to life. There are, therefore, two principal conditions which influence the life of warm-blooded animals deprived of air; first, the quantity of heat developed by the animals themselves, and, secondly, the external temperature to which they are exposed.

The fifth chapter in this part treats of respiration in youth and age. Like all its predecessors, it is based exclusively on simple and beautiful experiments. The re-

sults thus obtained oppose all preconceived opinions, and suffice to establish the important fact, that in warm-blooded animals the consumption of air is at its minimum in youth, and increases with age, and always bears a strict relation to the capacity of generating heat. Thus adult sparrows consume in one hour and thirty-two minutes as much air as young sparrows of equal size in two hours and thirty-nine minutes. But the capacity of generating heat is greater in the adult than in the immature bird. Again, experiments having already decided that the generation of heat is greater in winter than in summer, it might be expected, from the preceding facts, that the consumption of air would be increased in the cold season. Experiments were performed on yellow-hammers to decide this problem, and it was found that each adult bird consumed as much air in fifty-two minutes in January, as it did in one hour, eight minutes, and fifty-five seconds, at the end of August.

The concluding chapter in this part is occupied by the examination of "perspiration or exhalation" as influenced by the external physical agents, of the hygrometric state of the air, its motion, or rest. The effects produced by these conditions were strictly analogous to those obtained in the experiments on cold-blooded animals which we have already noticed. The same law of loss by perspiration in equal and successive periods was found to govern both classes.

Some notes on the third part, which we have thus analysed, are contributed in the appendix, by Dr. Marshall Hall and Dr. Hodgkin. The latter physician also inserts an analysis of some speculations by Dr. Calvert Holland, on the subjects of Dr. Edwards's experiments. When we add, however, that Dr. Hall explains everything difficult by the aid of something which he calls "irritability," and that Dr. Holland gravely lays it down as an axiom, "that the generation of heat is in the inverse proportion to the quantity of blood in the lungs," we think we may spare ourselves the trouble of condensing the commentaries of these gentlemen on Dr. Edwards's opinions. Dr. Hodgkin, indeed, openly expresses his conviction that they leave Dr. Edwards's results altogether untouched.

In subsequent numbers of THE LANCET we shall proceed with our close analysis of this admirable book, so that those who do not become possessed of the work itself, may find its miniature in our pages.

Journal de Pharmacie, et des Sciences Accessoires, &c. November, 1832.

AN unusually valuable number of this journal has just been published. Three papers amply entitle it to this compliment. The first, one on opium by the well-known and indefatigable Pelletier; another, on "The influence of the presence of water in numerous chemical decompositions," by M. Pelouze, one of the most distinguished members of the Polytechnic School; the third by M. Sarzeau, on "The presence of *copper* in organic matters," a subject which has more than once engaged the attention of the readers of THE LANCET.

OPIUM.—PELLETIER'S LATE DISCOVERIES.

M. Pelletier's paper is of considerable length, but of more than commensurate value. It contains the best and most interesting analysis of opium yet published; it signalizes the presence of at least three *new* principles in that complex but most important substance; it improves our knowledge of all the principles previously discovered; it elucidates and explains much of the obscurity respecting the agency of tests and re-agents on its various solutions; and, finally, it establishes clear and satisfactory data from which the medico-legal chemistry of this poison must inevitably derive the greatest improvements, as well in the delicacy as in the certainty of its operations. M. Pelletier's essay was read before the Academy in August, and of course became only the subject of vague and imperfect notices in the public journals. The copious analysis we subjoin will, however, obviate the many misconceptions which these erroneous reports have unquestionably occasioned.

The paper consists of three divisions; the first of which constitutes an introductory sketch of the labours of preceding chemists

on this subject. We pass this by, however, as the works of Duncan and Thomson have already made this department of the history of opium fully known to the British practitioner and student. The original part then forms two sections, in the first of which the author gives an exposition of the process he employs in the analysis, and which is so devised as to enable the chemist to separate the twelve principles of opium during the course of one operation. In the second he discusses many interesting points in the history of the several principles thus obtained.

PART I.—MODE OF ANALYSIS,

(*Condensed from the author's description*).

A kilogramme of dry opium of Smyrna, was divided and macerated in two kilogr. of cold distilled water; the action of the solvent being assisted by kneading with the hands. This maceration is frequently repeated, the liquors filtered, and carefully evaporated to dryness. This product is termed "*extract* of opium." The substance insoluble in water is called "*marc* of opium." The proportion of the former to the latter is generally as twelve to four.

Examination of the Extract of Opium.

This extract was again treated with distilled water, it was nearly redissolved, leaving, however, a brilliant crystalline matter, which, washed, dried, and dissolved in alcohol, proved to be *narcotine* free from any trace of morphine. We shall presently find, however, that the greatest part of the narcotine remains in the *marc*. The solution thus freed from narcotine was then heated to 212°, and ammonia dropped in in excess, and the boiling continued for ten minutes. It was then slowly cooled, and the *morphine* separated in the crystalline state. If the liquid be filtered while boiling, a considerable quantity of well crystallized morphine is obtained. That which on cooling remains on the filter is, on the contrary, black, and mixed with resinoid matter.

But the morphine separated thus by ammonia, is far from being pure; when treated with ether it colours it yellow, and yields an oily matter, crystals of narcotine, and a new substance named *meconine*, to which we will again advert. The best way, however, to separate morphine from this narcotine for commercial purposes, is to redissolve it in the smallest possible quantity of dilute sulphuric acid; the narcotine is thus left, and the sulphate of morphine may afterwards be decomposed by magnesia, and the morphine separated by alcohol.

The liquid precipitated by ammonia was now reduced to one-half by evaporation. On cooling, more morphine was deposited. Water and baryta were now poured into the fluid, and a precipitate was obtained of the meconate of baryta. *This was boiled in alcohol repeatedly* (an important improvement, by which brown colouring matter is separated from the meconate of baryta) and the baryta separated from the *meconic acid*, by means of dilute sulphuric acid. The solution of the *extract of opium*, thus deprived of morphine, narcotine, and meconic acid, was treated with sub-carbonate of ammonia, to free it from any excess of baryta, and then boiled to expel the excess of carbonate of ammonia. It was next evaporated to the consistence of a thick syrup, which was left aside for several days in a cool place, where it was found to have become a pulpy mass covered with crystals. Having been compressed between linen, it was heated with boiling alcohol, by which it was partly dissolved, leaving a black viscous matter, to which we will again return. The alcoholic solution having been distilled to a small volume, deposited, on cooling, a crystalline substance, which was readily purified, and obtained in brilliant white crystals. *This substance is altogether new*, differs completely from morphine and narcotine, and has received the name of NARCEINE. The black viscous matter again treated with ether, is partly dissolved, and the otheral solution yields crystals of *another perfectly new substance*, which was discovered by my assistant, M. Couerbe, and named MECONINE. This meconine is associated with the narceine as described, but is readily separated from it by ether, which dissolves the meconine and leaves the narceine. Meconine possesses many singular qualities, which its discoverer, M. Couerbe, will detail to the Academy in a separate memoir.

The black viscous matter left after the separation of the narceine and meconine, being but a part of the *marc* dissolved in water by the influence of the other principles of the opium now separated, its examination is included in that of the *marc*. It may, however, be stated here, that in this black substance, as well as a colouring matter, gum, and minute quantities of the other principles of opium, there exists *a peculiar acid*, to which we shall again advert.

Examination of the Marc or Residuum.

This was treated with warm alcohol, the solution allowed to cool, and *then** filtered. The filtered liquid, distilled to three-fourths, furnished, on cooling, a considerable

* If filtered while hot, some caoutchouc would be deposited on cooling. The caoutchouc, by this method, was left in the marc.

quantity of narcotine. The remaining fluid was evaporated to dryness; the residue was taken up by boiling alcohol, and on cooling still afforded narcotine. A soft, fatty, mass, of a dark brown colour, remained, which was subjected repeatedly to the action of boiling water. The water separated a little narcotine and gum. The unctuous matter, thus completely deprived of substances soluble in water, dissolved completely in alcohol. Treated by sulphuric ether, it separated into two parts, one soluble in ether, the other brown, friable, and insipid. The ethereal solution evaporated, affords an *oily substance* well worth attention. It is soluble in alcohol and oil. Its taste is acid and burning, a circumstance which entitles us to presume that it is not destitute of action on the animal economy.

The part of the marc which was insoluble in alcohol, was treated with sulphuric ether, which became strongly coloured. By the spontaneous evaporation of the ether, and the subsequent washing of the residuum with warm alcohol, pure *caoutchouc* was obtained. From all these experiments it is evident, that of the substances extracted from the marc of opium by alcohol and ether, the *resin* is soluble in alcohol, and insoluble in ether; the *caoutchouc* soluble in ether and not in alcohol; and the oily matter soluble in both these fluids.

After all these operations the marc was reduced to a very small volume, which was found to consist of lignin gum, and a gummy matter of similar properties to that of bassorine. No starch could be detected. To sum up then, opium consists of the following twelve substances. Narcotine, morphine, meconic acid, *meconine*, *narceine*, a brown acid and extractive matter, *a peculiar resin*. *Thick oil*, gum, bassorine, and lignine. Of these the substances, the names of which are printed in *italics*, are now recognised for the first time.

Such is a careful epitome of the first part of M. Pelletier's admirable essay. We present an equally instructive condensation of the second, in which he makes some of the principles above enumerated the subject of particular observation.

PART. II.—OF NARCEINE.

Pure narceine is white and silky, in delicate elongated four-sided crystals when crystallized from alcohol; in crystals of a flatter shape when obtained from water. It is free from smell, and has *a galvanic taste*, like that produced by the contact of zinc and silver in the mouth. It is soluble in 230 parts of boiling and 375 of cold water. It fuses on a slight increase of temperature. On increasing the heat it is decomposed without sublimation. Concentrated mineral acids destroy it, and the diluted mineral acids combine with it as with a salifiable base of the second order.* But the combination of narceine with the diluted acids, gives rise to very remarkable phenomena.

When, for instance, muriatic acid diluted with one-third of water touches dry narceine, this substance takes on a magnificent blue colour. If water be added so as to effect the solution of this mass, it becomes altogether colourless. By evaporating cautiously to dryness, the blue colour reappears: no matter how the *water* is removed, the blue colour is equally produced. Thus the chloride of calcium (dry muriate of lime) absorbs the water of the solution and becomes blue. All the mineral acids, and several of the vegetable, produce the same effects. The metallic solutions are destitute of any marked action on the combinations of narceine. Solutions of iron are known to strike a blue colour with *morphine*, but they have no effect on *narceine*; consequently the blueness produced by acids on the latter, is perfectly distinct from the agency of iron on the former.

The phenomena produced by heating narceine strongly with the naked flame, are very remarkable; it swells up, produces vapours, first white, then yellow. In the receiver is found an acid liquid, of balsamic odour, and, in the neck of the retort, acid crystals, soluble in alcohol and water, and striking a blue colour with solutions of persalts of iron (gallic acid?).

On ultimate analysis, narceine was found to consist of

Carbon	54.73
Azote	4.33
Hydrogen	6.52
Oxygen	34.42

* M. Pelletier considers that there are three orders of organic salifiable bases. The first forms *neutral* salts; the second crystalline salts, with *excess* of *acid*; the third are the organic alkaline substances, which, nevertheless, cannot saturate acids. *Quinine* exemplifies the first, *delphinine* the second, and *narcotine* the third of these orders.

We pass by the section relative to narcotine, and proceed to the following, in which M. Pelletier communicates the results of some deeply interesting experiments on the

Cause of the Blue Colour struck by Morphine and the Results of Iron.

Up to the present time it has remained unknown, whether this blue colour was owing to a combination of morphine with the salts of iron without alteration of the morphine, or whether the morphine is changed, and gives rise to a new combination. The following experiments determine the nature of the actions:—

Dry morphine was treated with a concentrated and neutral solution of permuriate of iron, and it immediately assumed a deep blue colour. In twenty-four hours the entire had become a crystalline colourless mass, but on gradually adding water, the colour returned, and not an atom of oxide of iron was precipitated. On evaporating the solution, very pure muriate of morphia was obtained. By exposing the blue-coloured solution to the air, peroxide of iron is formed, and decoloration ensues.

From these facts M. Pelletier concludes, that in this reaction a part of the morphia attracts a portion of oxygen from the peroxide of iron, and then unites with another portion of protoxide of iron, forming a *morphite* of that metal. The following experiment confirms this theory.

The blue fluid, after it had ceased to yield muriate of morphia, was evaporated to dryness, and left a brownish deliquescent mass; this, treated by alcohol, was partly dissolved, there remaining a matter soluble in water, and giving it a violet colour. The alcoholic tinctures, evaporated spontaneously, furnished a violet substance; this was partly dissolved in ether, and the solution took a greenish colour. By spontaneous evaporation this again furnished two substances; one, not crystalline, was the chloride of iron, the other was in crystals, the minutest particle of which sufficed to colour a large quantity of water deep blue. To this substance, then, is to be attributed the cause of the colouration.

Resin of Opium.

This, M. Pelletier has found to differ from the majority of other resins. 1st. By its insolubility in ether; 2nd. By its containing azote; and, 3rd, by its atomic constitution.

Oily Matter of Opium.

This should be considered as a fatty acid analogous to the oleic acid. It unites with potass and soda to form soaps; it contains no azote, but consists of

Carbon	72.39
Hydrogen	11.82
Oxygen	15.78

An ultimate analysis of the caoutchouc of opium is added, by which the identity of this substance with the caoutchouc of commerce, is completely established. M. Pelletier passes over the meconic acid, in consequence of M. Robiquet's being engaged on the subject at present. The bassorine, gum, and lignin, are passed over as being of too little importance to deserve especial notice.

We have thus completed our analysis of M. Pelletier's paper, which occupies thirty-five pages of the journal.

PRESENCE OF WATER IN CHEMICAL DECOMPOSITIONS.

We now arrive at the memoir by M. Pelouze, of the Polytechnic School, on the influence exercised by the presence of water in a great number of chemical decompositions. The first fact of this kind noticed by the author is extremely curious. Concentrated acetic acid does not decompose common carbonate of lime, even when assisted by a boiling temperature, and increased pressure of ten atmospheres. Again, he found that if a certain quantity of water saturated with carbonate of potash be introduced into a tube, and then a mixture of acetic acid and alcohol added, the carbonate of potash is precipitated as a white powder, without the extrication of a particle of carbonic acid. Thus the presence of alcohol modifies the affinities to an extraordinary degree. But the converse of the experiment is still more remarkable. An alcoholic solution of acetate of potash was placed in a tube, and carbonic acid passed through, when carbonate of potash was thrown down. Thus, under the same circumstances of pressure and temperature, carbonic acid decomposes the acetate of potash, and acetic acid the carbonate of potash. The author further shows that acetic acid dissolved in alcohol is devoid of action on litmus. The former remarks also

apply to the tartaric, paratartaric, and oxalic acids. How material these important facts may be, during the manipulations of organic and medico-legal chemistry, the practical analyst will at once understand.

COPPER IN THE BLOOD, &c.

M. Sarzeau's article on the presence of minute quantities of copper in organic matters concludes this number. We several months since published the results of M. Sarzeau's early experiments, lately corroborated by a British chemist, and sufficient to establish the interesting fact which the title of the present essay describes. Some doubts having been thrown by M. Chevreuil on the accuracy of M. Sarzeau's observations, he has repeated, modified, and extended his experiments, and arrived at the fullest corroboration of his former statements. Copper must, consequently, be for the future enumerated as a constituent of the blood as well as iron, and the earthy and alkaline salts. In addition to the mode of analysis he formerly recommended, and which has been already published in THE LANCET, M. Sarzeau in this article dwells on the blowpipe as an agent of wonderful delicacy in the detection of copper. These important facts must evidently be carefully remembered in medico-legal investigations relating to the cupreous poisons.

Lectures on Anatomy; interspersed with Practical Remarks. Vol. IV. By BRANSBY B. COOPER, F.R.S., Guy's Hospital, &c. London. Longman 1832. Roy. 8vo. pp. 383. Plates.

ALTHOUGH the previous volumes were noticed in this journal in a spirit of the utmost fairness, yet some puny and dabbling critic, not forgetting the cause "Cooper *v.* Wakley," had the temerity to assert, that our pen had been guided under a feeling of prejudice; whereas nothing could be more untrue, and Mr. Cooper has now himself proved the falsity of the accusation, by correcting several of the errors which we did not hesitate to allege were evident in the pages of the three first volumes.

The present volume is a continuation of the work; we notice here also several errors. We shall proceed briefly to point some of them out. The volume contains the descriptive anatomy of the arteries, veins, absorbents, nervous system, and the four sinuses.

As the basis of his nomenclature, Mr. Cooper has adopted the system first proposed by Mr. Joshua Brookes. This has many advantages, but frequently leads to the necessity of baptising a vessel with a name which becomes long-winded exactly in proportion to the insignificance of the branch, and which must be sadly perplexing to the "slenderly Latinized." Thus we have "Ramusculi recurrentes interossei anteriores" to designate some small twigs about the elbow-joint; and "Ramuli carpi ulnares anteriores et posteriores," are the proper names of a few branches on the carpus. It is not easy to account for the author's attachment to these long names, the more especially as his own Latinity is not very immaculate, witness "foramen auditorius internus," p. 241.

The physiological arrangement of Sir C. Bell is adopted by the author in his descriptive anatomy of the nervous system, and so far forms an improvement; but when Mr. Cooper enters into speculations of his own, he is sure to err, through want of accurate knowledge in what might be termed (though unjustly) the minor points of anatomy. Thus in commenting on Sir C. Bell's opinion, that the oblique muscles are involuntary, the author remarks, that the third nerve supplies the inferior oblique, and says, "Here is an instance of an involuntary muscle being supplied by a voluntary nerve; the ramus, however, which goes to this muscle is connected with the ophthalmic ganglion, and may therefore, perhaps, be considered as a compound nerve." Mr. Cooper could not have adopted this theory if he had been aware of the fact that the mode of connexion between the lenticular ganglion and third nerve is by no means constant; and that sometimes the short root of the ganglion is not derived at all from the motor communis, or any of its branches. An equal disregard to accuracy is shown in the description of the origin of the first cervical nerves, or sub-occipital, "which arises (p. 252), like all the other spinal nerves, by anterior and posterior filaments." Now every tyro in anatomy

knows, that the sub-occipital nerve, in the majority of cases, has no posterior roots, and is, therefore, separated by many writers (and by Mr. C. himself, without any assigned reason) from the cervical nerves.

The account of the posterior or external saphenal nerve, is altogether inaccurate in one place (p. 279), though, strange to say, its mode of formation is correctly given in another page (281).

"*Ramusculus nervosus cutaneus* (the communicans fibularis of most authors). This is sometimes distributed in two or three filaments, which descend along the outer and back part of the leg, between the gastrocnemius externus muscle and the skin, it unites with the cutaneous filaments of the posterior tibial ramusculus, together supplying the skin in their passage downwards to the malleolus externus, behind which they pass to terminate by dorsal filaments," &c.—p. 279, 80.

In this short sentence there are are no less than three anatomical errors. In the 1st place, the branch does not run between the gastrocnemius and skin, but between the fascia and muscle; and if the student were to seek it under the skin, and not under the fascia of the leg, he would never progress in his dissection. 2nd. The communicans fibularis does not unite "with cutaneous filaments of the posterior tibial ramusculus," but with the trunk of this nerve itself, near the outer edge of the muscle, just above the tendo Achillis. 3rd. *They* do not pass "downwards to the malleolus externus, behind which *they* pass to terminate," &c.; but they united form one considerable branch, properly called the posterior or external saphenal nerve; which nerve, as a single chord, not as a double trunk, passes behind, &c.

We would also remind Mr. Cooper, that it was first distinctly noticed and proved by the distinguished Scarpa, that the naso-palatine nerve (nerve of Cotunnius) does not pass down through the anterior palatine canal to the foramen incisinum, but through a very small and peculiar canal quite close to the former. In the sheep this is most distinctly seen; the naso-palatine penetrates the roof of the palate, nearly an inch behind the palatine canal.

The natural and pathological anatomy of the semilunar ganglia is doubtless familiar, ere this, to most of our readers; Mr. Bransby's description is either very carelessly drawn up, or very wrong.

"The great splanchnic nerve passes through the lesser muscle of the diaphragm into the abdomen, and immediately above the capsula renalis, it divides into numerous filaments to unite with the ramus splanchnicus major of the opposite side, through the medium of the great semilunar ganglion at the root of the cœliac axis." —p. 295.

From this description the student would naturally enough conclude, that there was only one semilunar ganglion, and that he would find it by dissecting down upon the cœliac axis. We can assure him that both impressions would be erroneous.

Perfection we do not expect, but we have a right to complain of such blunders as these. The work, however, has its merits, and now that we have enabled such youthful purchasers as may be obtained for it, to correct some of its errors, it is worth so much more than it was before the publication of our strictures.

THE LANCET.

London, Saturday, December 8, 1832.

A BRIEF report of the proceedings which took place at a meeting of medical students on Monday evening last, will be found in another column of THE LANCET of this day. The object of the promoters of the assembly was to address the SECRETARY of STATE, regarding the manner in which the parochial authorities are said to have abused the Anatomy Act, by unequally distributing subjects to the schools, thus calling on the students to petition Lord MELBOURNE to exercise his influence in securing to the different schools a distribution of subjects in a ratio to the number of pupils.

We regret to find that the meeting was a boisterous one, and that no resolution was adopted which was in any degree calculated to remove the difficulties which press so heavily upon some of the schools. Discussion in such a case, even if conducted

in the most moderate tone, could not be without its dangerous tendencies. The Anatomy Act is in itself odious to a large portion of the community. Even the *principle* of the measure, and the operation of the Act, are, in detail, held to be truly detestable. We question if the students were well advised in determining to hold such a meeting, because the SECRETARY of STATE for the Home Department, can exercise no authority over the parochial officers, whom the students are anxious should be controlled in their conduct. The source of the mischief lies deeper than is imagined, and no address to the SECRETARY of STATE can have the effect of accomplishing its removal. The Anatomy Act being a most ineffective measure, it is utterly impossible that it can work well, as regards a general supply of subjects to the schools of anatomy. As this new law permits the parochial authorities to dispose of the bodies of the unclaimed, either at a high or at a low price, it follows as a necessary consequence, that the subjects are sold in the best market, and the SECRETARY of STATE has no power to regulate the price, to limit the supply, or to direct in what quarters the distribution shall be made. THE LANCET foresaw all these difficulties, and commented on them fully when the bill was under discussion; but the voice of avarice was too influential for the dictates of justice and common sense; the bill passed into a law, in spite of all our remonstrances, and we are now suffering another sting under a new specimen of bad legislation.

Although it is much to be wished that the students would combine in a body in petitioning the legislature for a remodelling of this very stupid and obnoxious enactment, yet it can scarcely be expected that those young gentlemen who are furnished with an adequate supply of subjects, will make common cause with the members of those schools which are destitute of the materials for the successful study of anatomy. It is to be regretted, then, that any attempt of the kind should be made, as the effort can only lead to a farther separation of the divided parties, and to the production of scenes of disgraceful tumult. Of this we are quite certain, that if the public attention be drawn more closely to what is now going on under the Anatomy Act in the *supplied* schools, even the dissecting rooms of those establishments will be deprived of any considerable quantity of subjects. We repeat, that the ground is altogether a dangerous one, and cannot be trodden by angry disputants without extreme hazard. If the gentlemen belonging to the private schools were to direct a more searching scrutiny into the abuses which they are investigating, they would find another cause for an efficient supply of subjects to the hospital schools than an abuse of power by the parochial authorities. When the Anatomy Bill was first brought forward by Mr. WARBURTON, we complained, in no very measured terms, of the absence of a check on the officers of our hospitals as to the appropriation of the bodies of the persons who might die in the wards of those establishments. Are the students of the private schools quite certain that the hospital dissecting-rooms are *supplied wholly by the parochial authorities*? Ah! here is contained one of the secrets which the uninitiated have yet to learn. Here exists one of the glaring defects in that most defective of all measures, the Anatomy Act. Circumstances greatly deceive us if one-half of the supply to the hospital schools be not now derived from the wards of those institutions. The natural tendency of the law is to lead to such a result, and it would be absurd to believe that the teachers of anatomy in the great recognised charities, do not profit by the exertions of their contemporaries at the bed-side of the sick. The law, as it now stands, confers unquestionable advantages on all the great recognised schools, and as obviously operates to the severe detriment of

the smaller private establishments. There is NO PRINCIPLE OF JUSTICE in the measure. It embraces no enlarged view of the great position which the fundamental practice of medical education should maintain. Everything is left to chance, to caprice, to avarice, to envy. The power which is now *permissive*, should be made *peremptory*, and the welfare of *all* the schools—involving, as the study of medicine does, the health, and consequent happiness, of the community—should have been the great and undeviating aim of the law.

The present enactment is a mass of inefficiencies and incongruities, and, as it authorises, or, rather, as it does not interdict, the buying and selling of human bodies, it is viewed with feelings of abhorrence by the trading and labouring classes of the community. If this measure be not amended, or almost re-modelled, the teaching of anatomy will become a close monopoly in the great recognised schools. The favoured and wealthy teachers, always deriving, as they do, a large supply, gratuitously, of subjects, from the wards of their own institutions, will be enabled to give a very high price for the bodies which they may have occasion to purchase from parochial officers. Having thus established an exorbitant charge in the market, few bodies would be purchased unless at the maximum price, and thus the private teachers would be ruined, and their pupils driven to the larger establishments. Hence, competition amongst lecturers would be extinguished, and merit go altogether unrewarded.

What we contended for when the bill was before the House, and even long before that period, was this:—

That a person should be appointed by Government, to regulate the distribution of dead bodies.

That no bodies should be dissected in *any* school, without having been first exposed for two days, at least, in a house of general reception for the unclaimed dead. At the expiration of that period, the distributor should communicate to the conductors of the schools, in a just order of rotation, that a subject lies for removal; or the conveyance might be effected through the agency of the office.

Had this plan been adopted, students would not now be required to pay more for the subjects than the expenses incurred by the conveyance from place to place; and considering, as we do, that the study of anatomy is an affair of national importance, we are of opinion, that even this payment should be made by funds from the public treasury, and that the salary of the distributor should be derived from the same source. Such a plan affords the only security for equalizing the supply in proportion to the wants of the various schools.

Added to this paramount advantage, let us reflect for a moment on the security which would thus be afforded to the public against a repetition of the crimes committed by the BURKES and the BISHOPS.

In connexion with the arrangement here stated, a law should be enacted, rendering it a *transportable offence* for any surgeon, physician, or apothecary, or any other individual, to dissect a body which had not been obtained from the public receiving house, under the immediate sanction of the Government distributor. Thus the whole of the supply would be derived from one house, and that establishment placed under the effective control of an officer in the pay of the Government. Even this scheme, however, we should not regard as perfect, unless the Anatomy Act contained a clause which made it penal for any individual, no matter what his occupation, to buy or sell the dead body of a human being. The trade of the resurrectionist should be utterly abolished, and not carried on, as it is at present, by the teachers of anatomy. The public must be taught to regard the anatomy question as one of vast importance

to themselves. They must be made to understand that medical pupils delight not in human dissection, but that they examine the structure of the dead, in order to keep sound the structure of the living. Let them learn that the teachers of anatomy no longer make a profit by trafficking in human flesh, but that the bodies of unclaimed persons are placed with the utmost delicacy upon the table of the anatomist, and that after the demands of science and knowledge are satisfied, the scrutinized remains are deposited in the earth with the usual rites of Christian burial. Further, that no individual, whether rich or poor, who, before death, may have expressed a desire that his body should not be dissected, shall, in any instance, be submitted to the scalpel of the anatomist. If pupils and medical practitioners unite to attain the objects thus explained, success will inevitably crown their exertions at an early period of the next session of Parliament.

In conclusion, let us observe, that it is easy to utter complaints against the parochial authorities, but it is not so easy to understand the difficulties which beset those individuals who have the misfortune to occupy parochial offices. If the poor believe that their bodies are disposed of, after death, at a profit, by the keepers of the workhouses, it may be readily imagined that churchwardens and overseers are anxious to conduct themselves in such a manner as to lead to no suspicion that they have been engaged in the sale of the dead. This is quite certain,—that, under existing circumstances, nothing short of high prices could induce them to engage in a traffic at once so dangerous and so disgusting.

TORSION OF ARTERIES.

M. Fricke, head-surgeon to the General Hospital at Hamburgh, has addressed the following observations (since published in the *Archiv. Gen.*, Sept. 1832) to the Royal Academy of Medicine upon this subject.

"Shortly after M. Amussat published his experiments in the torsion of arteries, I determined to repeat them, and am now gratified to say, that I have had no cause to regret the trial. There are from 350 to 400 operations annually performed in the hospital of Hamburgh; and for three years, both there and in private practice, I have continued to employ the torsion. I have, with very few exceptions, practised this method on every artery, on the femoral, immediately below Poupart's ligament, and on the axillary, without any dangerous consequences, such as suppuration, secondary hemorrhage, &c.; several native and foreign surgeons have witnessed the application of torsion, and many of our Hamburgh surgeons now use it with success. M. Amussat's operation is somewhat more complicated than mine, which consists in seizing the extremity of the divided artery with a forceps slightly modified from the common one, and twisting it until the laceration of a small portion of the membrane has assured us of the success of the operation. In this manner I practised the operation more than one thousand times."

UNEQUAL DISTRIBUTION OF DEAD BODIES AT THE WORKHOUSES.

MEETING OF MEDICAL STUDENTS.

A meeting of the medical students attached to the London schools of anatomy was held on Monday evening last, pursuant to public announcement, in the great room of the Albion Tavern, Great Russell Street, Covent Garden. The conveners announced the object of the meeting to be to concert measures for remedying the present partial mode of distributing dead bodies from the workhouses among the schools, and to prepare a memorial to Lord Melbourne, containing facts in proof of the grievance, and praying him to remove all cause for complaint, under the powers with which he is invested by the Anatomy Act.

Long before eight o'clock the room was nearly filled by students. It was moved that Dr. Lynch should take the chair, and the great majority of the meeting appeared favourable to the proposition, when

Dr. Lynch came forward, and said that he was proud of the unexpected honour. He congratulated them on the independent feelings with which they had assembled, and hailed this determination of discussing their fellow pupils' rights, as giving promise that the present grievances would speedily be rectified, and lay the founda-

tion for ulterior proceedings, which would eventually emancipate them from the many evils under which the profession now laboured. They had assembled this evening with a perfectly justifiable object. It was the original intention of the framers of the Anatomy Bill, that the student should be provided with the means of acquiring a due knowledge of that science upon which medicine is based. (*Applause.*) The Government, in enacting this bill, never could have contemplated that some schools should have a superabundant supply of bodies, while others, who were equally desirous of pursuing their studies, should be debarred of that right, and have no supply whatever. (*Cheers.*) It would be monstrous to suppose them capable of such injustice. (*Applause.*) To the individual who objected to him as not being a pupil, he would answer that the oldest and most distinguished member of the profession is but a pupil in the school of scientific acquirement. He had himself been, for the last eight or ten years, in close connexion with the students of Dublin, Edinburgh, and Paris, and found them universally a generous class of young men, and he hoped that the pupils of this metropolis, the centre of intelligence, would not show themselves deficient in this respect, but scorns to be influenced by sordid and partial motives, and not suffer themselves to be made the tools of others, by offering a vexatious opposition to the proceedings on the present occasion. He disclaimed being actuated by party feelings, and called on them to respect principles more than persons. He had the benefit of all in view, but begged leave to decline taking the chair, lest it should interrupt the harmony of the meeting, or give umbrage to even a single pupil. (*Applause.*)

Mr. Morris then moved that Mr. Lunn (a student) should take the chair. Carried unanimously.

The Chairman then opened the business of the evening. He was aware that there were present several students who came from the large schools to which a great supply of subjects had been made. (Great interruption followed this remark, and the proceedings were at once stopped in such a manner, as to give evidence that some determination existed to defeat the object for which the meeting had assembled, and cries of "Turn him out" issued from many parts of the room against a particular individual.) At length, after many entreaties, silence was partially restored, and the chairman proceeded, though amidst continued interruptions from the person alluded to, and the party by whom it subsequently appeared he was supported. The Chairman called on the meeting to support him, and remarked, that any gentleman who continued to create a disturbance must be ejected from the meeting. Cheers came from one party, and hisses from the other, with great confusion, in the course of which an attempt was made to remove the disturber from the room, and the Chairman was almost inaudible. We understood him, however, to dwell upon the importance of anatomical dissection, and hoped that all who were present would manifest a liberal spirit by aiding to procure a remedy for the grievances of which their brethren had so much right to complain.

Mr. M'Crae read the first resolution, which was to the following effect, "that such medical pupils as consider themselves aggrieved by the present unjust and unequal distribution of subjects, on the part of the parochial authorities, do forthwith unite, and concert measures for the prevention of this abuse." He was rejoiced to see the pupils assembled with a view to deliberating for themselves; they had found out that this conservative system was not for their benefit, but for the benefit of the few, whose system was fast falling to decay. He was sorry that the objects of the Legislature, in the present instance, had been so maliciously perverted by those who ought to ornament the profession by their acts, but unfortunately were a disgrace to it. (*Confusion and outcry.*) The persons to whom he alluded had combined with the parochial authorities (*Shame, shame*), and it was well known that while *they* were well supplied with subjects, other schools, equally good, were left entirely destitute. (Cries of, "It's your own fault; keep to the point.") The remedy for this abuse was to be accomplished by the united efforts of the students to obtain redress. He hoped the resolution would meet with the approbation of the meeting.

Mr. Turner seconded the resolution, and was proceeding to address some comments to the meeting, when a rush was made at the table, which was occupied by the Chairman and gentlemen who were taking part in the business of the meeting. The individual (with whose name we are not acquainted) who conducted the opposition throughout the evening, with his friends, now endeavoured to usurp the places of those by whom the meeting had been got up, when a long struggle ensued. The uproar continued for nearly an hour, and to describe the scene which presented itself is impossible. The assailing party avowed themselves to be students of St. Bartholomew's Hospital. At length

Dr. Lynch again came forward, and a partial silence was obtained. He implored them to observe order, and evince some little respect for the office of the chairman, whose courtesy and efforts to carry the will

of the meeting into effect would have commanded attention at any other meeting; but here the proprietor of the tavern told them that the character of his house would be destroyed by the uproar which had occurred. He asked the discontents to place themselves in the position of those who convened the meeting, and consider whether they would not be highly indignant if other parties came in hostile array to drown the expression of their opinion, and by brute force drive them from the room. (The clamour now again began.) He made no particular allusions, but it would be want of manliness in him not to affirm, in reply to remarks, that he was justified in his observations, and that the meeting at one portion of the evening surpassed even the scenes of a bear garden. What effect must such outrageous conduct have upon the public mind?

The scene here baffled all description, and we can no longer attempt to delineate it. Dr. Lynch then recommended them to dissolve the meeting, and that the pupils who were aggrieved should attempt to effect their object by signing a petition which they might the next day request to have laid for signature at THE LANCET OFFICE, or some other central place.—(*Cheers.*)

After many fruitless attempts made by other gentlemen to obtain a hearing, the meeting broke up with a kind of chairing of the leaders of each party around the room. We should observe, that the numbers of the opposition were far greater than those on the other side, and that the "Bartholomew's men" proceeded on the avowed supposition that an "equal distribution" would be a positive loss to them, although the lesser schools might be benefited by a fair division.

BREACH OF PROFESSIONAL ETIQUETTE.

To the Editor of THE LANCET.

SIR,—About a month ago I was called to a case of strangulated femoral hernia, which, after a few minutes manipulation, I succeeded in reducing, and having thrown up a copious enema, I gave my patient strict orders not to leave his bed till he had furnished himself with a truss. Mr. Dashwood, "surgeon to the Southwark Truss Society," to whom a letter from a subscriber was sent, refused to give an order to the instrument-maker without first seeing the patient, observing, that if the man could not walk as far as the Borough, his (Mr. Dashwood's) fee would be a guinea for a visit. Messrs. Maudsley and Co., the well-known engineers, hearing that there was some difficulty in procuring a truss, and not being aware that my patient was out of danger, sent an order to Mr. Dashwood to see him. I met the gentleman, and after some observations on the subject of hernia, I handed to him the measure of my patient's pelvis, and parted on good terms. His subsequent conduct called forth the following notes:—

"To —— Dashwood, Esq.,
Surgeon to the Truss Society.

"Sir,—As I had by the taxis relieved my patient Hall from strangulated femoral hernia more than forty hours before you made your appearance on Thursday to take his measure for a truss, I am rather anxious to know for what purpose you visited him again yesterday in my absence. He was not in want of your assistance, neither was I. A written answer will oblige your obedient servant,

"J. F. HIGGINS.

"Lambeth, 8th Oct. 1832.

"To Dashwood, Esq.*

"Sir,—Since I wrote to you on the 8th instant, relative to a gross breach of professional etiquette, I have had strong reasons for believing that you have been guilty of a perversion of the truth in your report of my patient's case, and that you have endeavoured to detract from the merits of a brother practitioner.

"After calling on my patient in your capacity of surgeon to a truss society to take his measure, did you not go to his employers, Messrs. Maudsley and Co., uninvited by them, and state that their foreman was in great danger, although you were aware that I had relieved him by the taxis from strangulated femoral hernia, more than forty hours before you saw him, and that he was at the very moment you were making that statement free from any bad symptom?

"Did you not also state in the office of these gentlemen, that it was fortunate for the patient you were called in, remarking that I was unacquainted with the case; and did you not, from your observations, succeed in obtaining a fee, leaving your hearers under the impression, that you had rendered great service to the patient when, in fact, you had done nothing for him?

"If you can enter into a satisfactory explanation, I call upon you to do so, or I will hold up your conduct to the scorn of that profession of which you are, in my opinion, a * * *

"I shall offer no remark at present on the subject of receiving fees from the un-

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* Some terms, unusually harsh, in this note, have been omitted.—ED. L.

fortunate applicants to a *charitable* (!) society. I have not yet received an answer to my note of the 8th instant.

" I am, Sir, yours, &c.

" J. F. Higgins.*

" Lambeth, October 25th, 1832."

The following is the verbatim et literatim reply which I received.

" To Mr. Higgins.

" Sir,—You are really labouring under some Mistake as regards the statement, and I tell you this your Conduct to me, when you first met me, was Highly unprofessional, you must know I practice as a Consulting Surgeon, and am in the Habit of meeting Persons of High Character and it was a remark of mine at the time that you cou'd not be in the Habit of meeting in Consulting Cases.

" if you want any explanation as to my right in seeing the Patient, you must call upon me, I will then shew you a Letter from the firm.—I cannot Conclude without Bringing to your recollection, your remark about the External Ring and I sh'd like to know what it has to do, with Femoral Hernia.—you insulted me in the First Instance in Saying, " I was sent for to Measure the Patient for a Truss."—I should not have answered your Letter but I thought it wou'd settle the matter and therefore Sir I shall receive no Further Communication on the Subject

(Signed) "Jarrett Dashwood
Chir 85 High-street Southwark
" Oct xxvj, 32"

I need not observe, that Mr. Dashwood's letter is no answer to my last note. The *soi disant* consulting surgeon has not thought proper to take notice of the questions put to him on the 25th ult. * * *

I have the honour to be, Sir,
Your most obedient humble servant,
J. F. Higgins.

Lambeth, Nov. 19th, 1832.

LONDON UNIVERSITY MEDICAL SOCIETY.

To the Editor of The Lancet.

Sir,—Will you do me the favour to insert the present remarks in The Lancet? The following passage appeared in the Medical Gazette of last Saturday :—

" *London University Medical Society.*

" It appears that there is an association of pupils so called, and that they take in certain periodicals for their edification. Now Dr. Elliotson having declared war against the Gazette, it was very naturally thought by some of his pupils that it would be highly gratifying to their professor if they were to discontinue it, and accordingly a motion to that effect was unexpectedly proposed about a fortnight ago, and carried, notwithstanding the opposition of a considerable number of the members. Before their next meeting our answer to Dr. Elliotson had appeared, *when the former decision was reversed.* But we understand that the question is to be brought forward at the next general meeting."

It is not to be supposed that, in consequence of the " answer " of the editor of the above journal to Dr. Elliotson, the members of the Medical Society were influenced to change sides, and reverse their former decision. It was found that the manner in which the resolution had been carried, was in nonconformity with a rule of the Society, which provides that no private business of the Society shall be entered into, except at general meetings. The next of these takes place at Christmas, and so strongly are the majority of the members in favour of the expulsion of the Medical Gazette from their library, that I will venture to predict, with certainty, such a result of a motion to that effect, whenever it shall be brought forward.

It will be sufficient to say, in order to show that the measure does not proceed from a servile wish to flatter Dr. Elliotson, that the greater number of members do not attend that professor's lectures, but have made themselves acquainted with the merits of the question.

I am, Sir, yours,
A Member of the Medical Society.

MR. COSTELLO'S CURVED PERCUSSOR.

To the Editor of The Lancet.

Sir,—I beg you will have the goodness to give insertion to a line or two of explanation on the subject of the paper read by me before the Westminster Medical Society on the 23rd ult., and the debate which followed. I cannot at all account for the manner in which the facts contained in that paper were overlooked by some of the gentlemen who took a share in the proceedings of that evening; I acknowledge the spirit of justice with which they were, no doubt, actuated, but they seemed to be ignorant that a controversy had been carried on between M. Heurteloup and myself respecting the *curved percussor*, of which he claimed to be the inventor. I dissented on that occasion from his estimate of the value of the instrument, and gave the reasons for that dissent. I denied his claim to the merit of invention *in toto*. As you are aware, I addressed two letters on this subject to the Academy of Sciences of Paris. I did not take my objections on mere theoretical grounds, I learned its defects by experience. To the Westminster Medical Society, I pointed out those defects, I showed the steps which were successively taken, as in-

stanced in several models, to correct them, and, finally, I exhibited my present curved percussor, which some of the gentlemen present were pleased to call a masterpiece of simplicity and strength. I have no ambition to be called the inventor of an instrument which has been known, in form at least, so long, but I claim the merit of having rendered it a safe and valuable instrument, and I believe M. Heurteloup was made aware of my pretension, in a letter published in THE LANCET last July. I have used this instrument in the presence of a hundred medical men, who can testify for its efficiency and solidity,—in one case, where the stone was nearly three inches in diameter, and which had been subjected to the less safe percussor without yielding. If I had anything to fear by a comparison of my percussor with that of M. Heurteloup, I should not have provoked a comparison of them as I have done—not for the sake of a paltry triumph, but for the interests of science. I have the honour to be, Sir, &c.

WM. B. COSTELLO.

7, Parliament-st., Westminster,
Dec. 3, 1832.

WESTMINSTER MEDICAL SOCIETY.

Saturday, Dec. 1st.

Dr. COPLAND in the Chair.

NATURE AND TREATMENT OF EPILEPSY.

DR. EPPS brought forward his paper, containing the history and successful treatment of a *Case of Epilepsy of Twenty Years' Duration*. Dr. Epps related the circumstances nearly as follows:—A. B., æt. 34, the father of nine children, six of whom are now alive, has been subject to epilepsy since his eleventh year. The fits, which first occurred at intervals of a month, came on, more lately, once a fortnight, still increasing in frequency. He consulted me when at Manchester, in Sept. 1830. Several medical men of talent had been applied to, but the view of the case was very variable. One thought the fits arose from too great a flow of blood to the head, another from tapeworm—the former prescribing abstraction of blood, the latter *Ol. terebinth. rectificat.*, whilst a third had the resolution to confess his disinclination to undertake the case. Having tried so many plans without any permanent relief, A. B. almost despaired. He was, however, a man of intellectual pursuits, and, knowing a little of phrenology, came to consult me in the double character of phrenologist and physician, thinking that the two characters might be usefully combined in an inquiry into his malady. After a minute examination of his case, I told him, that though *many* symptoms militated against, yet *more* justified, a hope of recovery; and thus commenced the trial, which himself and his friends looked on as a final one. I may briefly state the patient's condition. He was affected with severe dyspepsia; excessive nervousness, frequent pains in the head, with soreness of the muscles of the neck; the fits occurring as before stated. The indications, therefore, were to relieve the dyspepsia, and remove the cerebellic affection. For the former I ordered the following mixture:—

Acidi hydrocyanici, ad form. domini Scheele, gtt. xxx;
Sulph. quininæ ʒss;
Sulphuris præcipit. ʒiss;
Confect. aromat. gr. xv;
Aq. distillat, f. ʒiij.

Of this mixture he was to take a teaspoonful three times a day, being cautioned to keep it excluded from air and light. To relieve the cerebellic affection I prescribed

Liniment. saponis, f. ʒj;
Linim. camphoræ comp. f. ʒss;
Tinct. lyttæ, f. ʒiv;
Ol. succini rectif. f. ʒvi.

This liniment was to be rubbed diligently down the spine every night. A. B. began attending to my regulations on Sunday, Sept. 26, 1830. During the first week he had great pain and weakness in the back (occasioned, as he thought, by the rubbing), also violent stitches, and sudden pains through the day, darting from the spine—a circumstance worthy of remark. I ought also to notice that he had experienced an attack of the epileptic fit a few days before I saw him. During the first week, already noticed, the nervous agitation was excessive, and on Monday, Oct. 4th, he compared the feelings in his head to "a whirlpool," and said that "although he had experienced an almost infinite variety of sensations, they were never so peculiar before," but, on the Saturday following, he felt so much better, "so light and free from pain, that he could fancy he had springs in his shoes." The occurrence of the peculiar feeling, on the 4th of October, may be referred to the circumstance of its being about the time of the bi-hebdomadal attack. The rubbing produced considerable eruption, troublesome to him on account of the itching, which I did not regret. A. B. found so much relief, that he gained great courage to persevere. The dyspeptic mixture and friction were used for a fortnight, the patient also bathing his feet, alternis noctibus, in hot water. Perceiving the expected effect was in progress of ob-

tainment, I wrote to desire him to desist from using the liniment for a week, after which he was to have recourse to it again, thrice a week for three successive weeks. The mixture (as the dyspepsia was much relieved) to be taken twice a day. Believing that the diseased condition was a want of power in the nervous fibres of the cerebellum, I ordered, in addition, a liniment, consisting of the *alcoholic extract of nux vomica*, dissolved in rectified spirit, to be rubbed on the occiput for eight days. This was commenced on Oct. 17, three days after which the nervous agitation before described recurred—this being about the bi-hebdomadal period. The patient felt little of it again till Nov. 6. Up to this period he had been free from fits during a very unusual length of time.

It may be proper to notice, that during the remission of the rubbing on the back, the shooting pains from the spine returned, but ceased on resuming the friction. Relief was also experienced from the friction on the occiput. In addition to the other means, I recommended A. B., with a view to the tonic effect, to wash the chest and neck every morning with cold water, rubbing them with a rough dry cloth. He was requested to disuse the head liniment fourteen days, and then to continue it for a week. During the interval from 28th Oct. to Nov. 6th, the patient was troubled with a *boil* "just below," as he states, "the organ of amativeness." In reply to his letter of the latter date, I desired him to take the following powder every morning:—*Sulph. quininæ* gr. iij; *pulv. cinnamonii* gr. j, and to rub the back every other week. This was attended to during eight weeks, after which the liniment for the head was used twice for seven successive nights. Between this and January 1st, he caught a severe cold, and had an attack of English cholera, the latter of which I am inclined to ascribe to the *quinine* acting peculiarly on the liver; to obviate this, he took Seidlitz powder, with a teaspoonful of the best Cognac brandy twice a week. The head I ordered to be rubbed with salt and water, and the back with the liniment, once a month for three successive nights. He now expressed great gratitude, having had no attack of the fits since I prescribed for him.

On March 4th, A. B. mentioned a circumstance illustrative of the modifications which dormant diseases often take on in the spring. He says, "For the last two months I have had a periodical attack, every fortnight, of *vomiting* in the night. It causes me to spring out of bed before it awakens me. The vomiting continues very violent for some time, and little, save a frothy matter, comes off the stomach." On one occasion the patient awoke before the vomiting came on, and the sensation experienced was that of a round substance at the bottom of the throat, which went down gradually till it reached the stomach. The vomiting then began. After the vomiting ceased, the round substance seemed to pass into the bowels, and then died away. This curious modification of the disease attracted A. B.'s attention so much the more, because he had not, previous to this, been subject to vomiting. Being convinced that the vomiting was periodic and spasmodic, I might say *hysterical* in its character, I ordered of the *liquor arsenicalis* three drops thrice a day, and the following pills—*Assafœtidæ lachrymarum* gr. xii; *Sulphat. quininæ* gr. viii; ft. pil. vi, duæ h. s. s. These were to be taken for the three successive nights before the expected attack. At the same time, with a view of keeping the bowels regular, I ordered pills composed of the comp. rhubarb pill and dandelion extract. The means prescribed were successful; still, however, between this and November 1831, A. B. was troubled with the suffocating feeling at intervals of two months, together with retchings and vomitings, the suffocating feelings continuing sometimes day and night. Looking, as I did, on these symptoms as indicative of the attempts of the vital powers to relieve the system from the affection of the brain, I considered it right to recommend the introduction of an *issue*. On March 23rd, 1832, A. B. wrote, and mentioned the good effect of this latter remedial agent. In the summer of this year he went on a journey, which still further promoted his cure. He is now entirely free from fits. It will be observed that I did not order the friction on the back of the head until I had tried and used the liniment on the spine. Finding the cerebral congestion removed by the spinal friction, I wished to try the effect of the tonic application—the solution of the alcoholic ext. of nux vomica.

On this case Dr. Epps remarked, first, that length of time is not an index as to the curability of epilepsy; secondly, that marriage did not remove the epilepsy, as it often does in persons of full habit—the seminal evacuations relieving the cerebellum; thirdly, that in cases of epilepsy attended with severe dyspeptic symptoms, a cure is more likely than in those not so attended. The peculiar modification assumed by the disease, and the benefit of the issue, were also objects of consideration.

Dr. Webster attributed the successful treatment of the epilepsy to the removal of the dyspepsia, and, indeed, the attention to diet was one of the very best remedial means that could be made use of. His experience convinced him of this, and he considered the fits, generally speaking, to be

immediately brought on by some food which the stomach is unused to, or incapable of digesting easily. He was little disposed to assign any potent effect to the liniment rubbed on the spine. He remembered a medical gentleman in the army, who had, through life, adhered almost totally to vegetable diet. This individual, when at the Cape of Good Hope, happened to be thrown into a situation amongst the Hottentots, where he was obliged to subsist on the flesh of a goat, and this eating of animal food was followed by a violent fit of epilepsy, from which he was near death. He (Dr. W.) had hoped that Dr. Epps would notice whether this patient had the aura epileptica. What was the character of the attacks? Was the patient insensible during the paroxysms?

Mr. Chinnock said, that Dr. Webster had anticipated the remarks which he himself had intended to make. The *issue* was often applied with success. Was it not so here?

Mr. Costello alluded to the cases published by Dr. Sharkey,* where this disease was treated with *foxglove*, and we understood him as affording corroboration of the success of Dr. Sharkey's practice. Mr. Costello agreed with Dr. Epps in referring so much to the state of the cerebellum.

Mr. Hunt observed, that the causes of epileptic fits were exceedingly variable. There were almost always premonitory symptoms, and those seemed to correspond with determination of blood to the brain. Though purgatives were successful in removing the disease for the time, yet he had always found a recurrence of it after. The important point in Dr. Epps's paper was the permanency of the cure. The grand thing was to discover and ward off the disposition to epilepsy.

Dr. Epps replied to questions from several gentlemen. He detailed the character of the seizures, which were like those usually met with. With regard to the dyspepsia, he had no hesitation in saying, that it was by combining the treatment of dyspepsia with the other means that the disease was conquered. Dyspepsia was not *per se* the cause of the epilepsy, but that condition of the brain existed in which originated the liability to it. The use of the strychine was directed to the debilitated state of the fibres of the cerebellum. His opinion was, that there are two causes of epilepsy connected with the cerebellum,—one where there is determination of blood in unusual quantity; another, in which the weakened fibre cannot even bear the afflux of the healthy proportion of blood. In the former he would use depletives, in the latter tonics, as in the instance of A. B. Dr. Epps thought the profession were much indebted to Dr. Sharkey. He had not tried his plan, but conceived a high opinion of its probability of success.

Dr. Johnson agreed with Dr. Webster that epileptic cases only admit of cure when arising from functional derangement. In a great number of cases which he had examined, he had never seen one in which the epileptic subject did not exhibit some deviation from the common appearances of the skull or brain. A frequent occurrence was, the thickening of the cranium, particularly where it covers the cerebellum. He then adverted to the use of the nitrate of silver, to which Dr. Webster had alluded in the course of the debate. Dr. Johnson said he had used it much, and could safely say that it did not act as a purgative in one out of twenty cases, nor would it produce, in the dose of five grains a day, any other sensible effect on the lining membrane of the intestinal canal than to allay irritation, and, perhaps, remove diarrhœa. It was almost the only remedy which he used in epilepsy, it being at the same time useful as an anti-dyspeptic, and as a counter irritant, to prevent congestion in the brain; for in epilepsy, whether proceeding from weakness or plethora, the immediate state of the brain, was turgescence of the blood-vessels.

Dr. Webster mentioned Dr. Powell's paper in the Transactions of the College of Physicians as bearing cases illustrative of the action of this remedy as an aperient. That was the experience which he had of it himself; his certainly did not correspond with that of Dr. Johnson.

Dr. Johnson. In what quantity do you give it?

Dr. Webster. Generally speaking, one-grain doses twice a day.

Dr. Copland observed, that very much of the variable effect produced, depended on its combination with other agents. It would check a diarrhœa dependent on want of tone.

Dr. Johnson said, he should commence giving one grain to children nine or ten years of age, and soon increase it to five grains. With regard to tinging the skin, he would expect it to happen if the remedy were continued longer than two or three months. This was the period allowed by Dr. Baillie. In his own practice, one gentleman, about to be married, found so much benefit from the argentum nitratum, that in his anxiety he continued it four months, and the consequence was, that his skin had the peculiar hue.

Mr. Costello referred to the length of time at which this effect of the remedy sometimes becomes obvious. He men-

* In The Lancet.

tioned one gentleman, related to a celebrated public character, whose skin did not become tinged till two years had expired. In reply to a question from Dr. Johnson, how long the medicine was taken, Mr. Costello said, "Five or six weeks."

Dr. Epps desired to know of Dr. Johnson, in consequence of his first observation, whether Dr. Johnson had any means of coming to a conclusion that a case of epilepsy proceeded from *functional* or *structural* derangement.

Dr. Johnson did not deem the distinction one of much consequence. In any case he could only hope to treat functional disorder. If a young person consulted him, he would commence with a sanguine hope; but, if the disease were of long duration—intellects impaired, memory affected, with dulness of apprehension, giddiness, and depression of mind, then he should expect to find disorganization of the cranium or brain.

Mr. Barnett and Dr. Johnson differed on this point, the former thinking it highly important to the practitioner to be able to distinguish, for the sake at least of his own peace of mind, between those cases where there is hope, and those where all is lost.

Some sparring ensued, in the midst of which the members rose to take their departure, when Dr. Copland announced, that on next Saturday the instruments of the lithotritists would be brought forward for critical examination.

MEDALS IN HONOUR OF A KNOWLEDGE OF BOTANY.

On Tuesday the annual medals for botany were thus distributed to the successful candidates by the Society of Apothecaries:—

The gold medal, to Mr. Thos. H. Cooper, of Lewes, Sussex, student of the London University.

The silver medal, to Mr. Philip King Weston,* of Boxhill, Surrey, student of King's College.

The gold medal had on one side the head of Linnæus, and upon the other the figure of a youth (possibly emblematical of the successful candidate), lying at the feet of Flora, who is recommending to his notice, Ray, Linnæus, Jussieu, and Sloane, whose names are engraved upon a monument by her side. The whole is well executed; its nominal value is 10*l*., and is certainly worth, at least, that sum: it is by Wyon, of the Mint. The silver one is from the same die. The giving of these medals is one of the very few acts, if not the only one, which the Apothecaries Company contributes to promote the zeal of medical students; and this causes much emulation amongst them. This year was one of more excitement than usual, in consequence of the examination being the first at which the King's College students have attempted to contest the gold medal with those of the London University, who have carried it off each year it has hitherto been given; this making the third time.

The answers to the questions given by Mr. Cooper, the successful candidate, were in the highest degree creditable to that gentleman.† The manner, also, in which Mr. Griffiths ‡ and Mr. Valentine distinguished themselves in displaying their knowledge of vegetable anatomy was highly gratifying to all parties. Indeed, the gentlemen of the London University are, we understand, all of them good botanists, and the King's College men will have hard work to beat their contemporary students, and be so mean botanists either.

* Mr. Weston obtained the first prize at King's College last year.

† There were forty-four questions, for the replies to which, three hours were given.

‡ Mr. Griffiths obtained the gold medal last year.

LONDON HOSPITAL.

REDUCTION, FROM FRIGHT, OF STRANGULATED OBLIQUE INGUINAL HERNIA.

A man, apparently 38 years of age, was brought to this hospital on the afternoon of the 3d November, labouring under a large strangulated inguinal hernia of the right side, which had been down about four hours. It appeared that for some years past he had had a reducible hernia on this side, but from which he was not much inconvenienced, excepting once, when he was unable to return it, and he in consequence sent for a surgeon, who effected its reduction.

This afternoon it again descended, and being unable to return it, he applied at this hospital for relief, where the taxis was employed for some time without success, when he was bled and put in the warm-bath, and while in the bath the taxis was again employed. These means failing and the symptoms being considered urgent, there being considerable prostration of strength, with vomiting and hiccough, although the tenderness of the scrotum was not considerable, Mr. Andrews (the surgeon for the week) was sent for, by whom the taxis was again employed for a short time without success, when finding all attempts at reduction were ineffectual, he proposed the operation to the patient, who obstinately refused to allow it.

On the necessity of submitting to the

operation being strenuously urged, he became excessively alarmed; extreme exhaustion followed, and while in that state, the patient's hand being on the tumour, it suddenly passed up into the abdomen, thus obviating any necessity for the operation.

ENCYSTED TUMOUR OF THE LABIUM.

M. L., a widow, ætat. 40, was admitted into this hospital on the 24th Oct., supposed to be labouring under pudendal hernia. We gathered the following particulars of the case from a practitioner residing in the neighbourhood, by whom she was sent to the hospital.

About three weeks previous to her being sent here, while attending her husband during his illness, she attempted to lift him in his bed; while so engaged she felt something give way in the abdomen, which was followed by this tumour. The tumour was at first small, but gradually increased until it attained the size of a hen's egg, and when seen by this gentleman she was labouring under obstinate costiveness, nausea, and vomiting; great tenderness of the abdomen with hiccough; there was also a certain degree of impulse communicated to the tumour on coughing.

On her admission here an examination was made by Mr. Scott and Mr. Adams, who considered it not to be pudendal hernia but a case of encysted tumour of the left labium. Sir W. Blizard and Mr. Luke being in the ward at the time, were consulted, and their opinion agreeing with that of the former gentlemen, the operation of removal was proposed; the patient consented to the operation, and was accordingly taken up-stairs to the operating theatre. The pubes being shaved, Mr. Scott proceeded to dissect the tumour from its adhesions. On carefully exposing the cyst it presented a dark congested appearance, such as led many gentlemen present to consider it an incarcerated intestine. On proceeding farther in the dissection, an incision was accidentally made into the cyst, when a quantity of grumous, pultaceous matter escaped, after which the whole of the cyst was removed.

After being placed in her bed, she was ordered to keep rags wet with water constantly applied to the labia, and to take the following pill.

Calomel, gr. j;
Cathartic extract, gr. v. *Ft. pil. Omni hora sumend. donec alvus respond.*

On visiting her in the evening, found she was free from any febrile symptoms: there had been a slight degree of oozing of blood from the incision, but not to any extent.

25th. Took four pills before the bowels were acted upon: they have now been freely relieved twice; skin cool; tongue slightly furred; pulse quiet.

26th. Has passed a good night: bowels open; tongue clean; pulse quiet; skin moist. The wound suppurates freely; to apply a poultice to the part.

30th. In every respect doing well. The wound discharges freely; healthy granulations are springing up. To discontinue the poultice, and apply a lotion composed of the chloride of lime and water to the part.

From this time nothing requiring particular notice occurred; the wound was gradually filling up by granulations, and the woman was discharged at her own request on the 15th instant.

SYPHILITIC IRITIS, ACCOMPANIED BY PUSTULAR ERUPTIONS.

R. H., sailor, ætat. 34, was admitted on the 4th of October, under the care of Mr. Scott, on account of diseases of the right eye.

On examining the eye there was observed to be increased vascularity of the cornea, and also of the tunica sclerotica. The distinction between the two orders of vessels was remarkably apparent; those on the sclerotica were passing in straight lines, which converging from all sides of the globe formed at the base of the transparent cornea a complete vascular zone, having a brownish appearance. The cornea was slightly hazy; the iris was much changed in appearance, being remarkably dull and muddy; at its margin was a tubercle about the size of a grain of mustard-seed, and an effusion of pus in the bottom of the anterior chamber. The pupil was very much contracted, and irregular, and but faintly obedient to the stimulus of light, which was intolerable, and produced a flow of scalding tears. The patient stated he had considerable pain in the globe of the eye, which also extended to the temple and forehead. The pain was most severe in the evening, but materially abated towards the morning. He said the right eye had been inflamed about two weeks previous to applying here, and he had not taken anything for it, excepting during the week previous to his admission, when he had leeches applied daily to the eye as an out-patient. The arms and face were beset with eruptions of a pustular kind, the chest and abdomen were free.

He was ordered to be cupped on the temples to ten ounces, and apply a blister to the nape of the neck; to take a scruple of calomel and jalap every other morning.

9th. The symptoms appear to be somewhat mitigated. The patient still complains of great intolerance of light, and the vascularity of the eye is great; has been

freely purged by the calomel and jalap: to take a drachm of the extract of sarsaparilla three times a day.

12th. The inflammation of the eye is rapidly subsiding, the effusion into the interior chamber is rapidly disappearing, but the pupil is still irregular. To continue the sarsaparilla.

16th. On going round the ward to-day Mr. Scott was informed he had chancres on the penis; these primary symptoms led Mr. S. to order five grains of blue pill night and morning, otherwise he did not intend giving any mercury in this case. The inflammatory symptoms have continued to subside, as also the eruptions, which have now nearly disappeared.

23rd. All the signs of inflammation are gone, and the eye is clear, the pupil remains irregular; his mouth has become affected by the mercury. To continue the pill and sarsaparilla.

30th. The eye continues quite free from inflammation; the pupil is still irregular, but is obedient to the stimulus of light, there is, however, some defect of vision; has had a slight attack of gout, for which he was ordered six leeches to the great-toe. Mr. Scott on visiting this case, observed to the pupils, "You cannot be surprised at mercury bringing out any disposition to disease which might be in the constitution; you see it is the case in this instance, and is frequently observed."

The patient from this time has continued free from any inflammatory affection of the eyes, and has been kept in the hospital on account of the chancres, which have done well.

CORRESPONDENCE BETWEEN PROFESSOR ELLIOTSON AND DR. A. T. THOMSON.

THE following notes have been addressed to Professor ELLIOTSON, by Dr. A. T. THOMSON, as "Dean of the Faculty of Medicine," in the University of London, in reply to inquiries from the learned Professor as to whether the assertion contained in the publication called the Medical Gazette, as to the "Faculty of Medicine" having come to a particular determination respecting the future introductory addresses, was true, and whether there was the slightest ground for making any such statement:—

"Tuesday evening, 4th Dec. 1832.

"The Dean of the Faculty of Medicine, in the University of London, presents his compliments to Dr. Elliotson. No such determination as that stated in Dr. Elliotson's note, namely, "that henceforth no opening address shall be permitted to be delivered until it has been laid before the Faculty, in order that the nature of its contents may be previously ascertained," has been adopted by the Faculty. No proposition for such a resolution has been made in the Faculty; and, consequently, no resolution of the kind stands on the minutes of the Faculty."

This not being sufficiently explicit, the Professor dispatched a second inquiry to Dr. A. T. T., which produced the following reply.

"Dr. Thomson presents his compliments to Dr. Elliotson. He does not exactly comprehend Dr. Elliotson's question. He knows of nothing which occurred in the Faculty of Medicine which could authorise the statement made by the Medical Gazette.

"3, Hinde-street, Wednesday morning."

PRINTER OF LONGMAN'S MOCK LANCET.

Mr. WILSON, the printer of the publication called the Medical Gazette, requests us to insert in this journal a contradiction of the statement of Professor ELLIOTSON in his late address to his class, that he (Mr. Wilson) had informed the learned Professor that his lectures had "doubled the sale" of the work in question. He adds, "that he is far from supposing that Dr. Elliotson attributed to him the above statement with the knowledge that it was incorrect." The contradiction reached us too late to be made last week. Of course, the *printer* of a pamphlet knows no more of its *sale* than the man in the moon, if he be not also the publisher. It is a common practice for authors, editors, and booksellers, to print twice or three times as many copies of their productions as are afterwards sold. Mr. Wilson (who, we doubt not, is a very respectable man) may be readily forgiven even if he did act the part of puffer to his employers on the occasion referred to. He has evidently, however, now forgotten what passed.

SUBSCRIPTION FOR DR. RYAN.

[The following list of subscribers is, at the request of Dr. Ryan, most willingly inserted in the pages of this journal.]

The following Members of the Medical Profession, and the Public generally, have subscribed, to aid Dr. Ryan in defraying the Law Expenses:—

	£	s.	d.
Dr. James Johnson, Phys. Extra. to the King.....	10	10	0
Dr. Uwins, Lect. on Medicine..	2	2	0
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G.	1	1	0
Dr. Aldis, Burlington-street	1	1	0
Dr. Jewel, Lect. on Midwifery	1	1	0
T. Radford, Esq., Manchester	2	2	0
A.	1	1	0
Professor Graves, Dublin	1	1	0
Professor Montgomery, Dublin	1	1	0
Dr. Leahy, Dublin	1	1	0
Dr. Harty, Dublin	1	1	0
Professor Apjohn, Dublin	1	1	0
Dr. Stokes, Lect. on Med.	1	1	0
Dr. Fergusson, Dublin Lying-in Hosp.	1	1	0
Dr. Collins, ditto	1	1	0
Dr. Breen, late Phys. to ditto	1	1	0
Dr. J. Labat	1	1	0
Dr. Maurice Collis	1	1	0
Dr. Churchill, Stephen's Green, Dublin	1	1	0
Messrs. Hodges and Smith	2	2	0
A True Friend	1	1	0
W. D. Mayne, Esq.	1	1	0
Dr. Cusack, Pres. R. Coll. Surg., Dublin	1	1	0
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J. Mahony, Esq., Pulteney-st.	1	1	0
W. J. Rose, Esq., Surgeon	1	1	0
Dr. Copland, Phys. Queen Charlotte's Lying-in Hospital	1	1	0
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Professor Lizars, Edinburgh	1	1	0
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Dr. J. Sanders, Edinburgh	1	1	0
W. J. S.	1	1	0
Amicus Justitiæ	1	1	0
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Dr. John Hancock, City-road	1	1	0
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Dr. Fergusson, Dep. Insp. Hosp. Windsor	5	0	0
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S. Plumbe, Esq., Surgeon, Southampton Row	1	1	0
Sir W. Beatty, a Phy. of the Fleet	0	10	6
N. B.	0	10	0
E.	0	10	0
F. H. S.	0	10	0
Earl Stanhope	5	0	0
Tho. King, M.D., Hanover-st.	0	10	0
The Satirist	1	1	0
H. Jay, Esq., Surg., Sloane-st.	0	10	0
J. M., Dublin	2	0	0
A Friend to Truth	2	0	0
G. B.	0	10	0
Dr. Quain	2	0	0

The Committee appointed at the meeting of the medical profession to apply to their brethren to aid in liquidating the excessive damages against Dr. Ryan, earnestly solicit their provincial contemporaries to assist them, and they hereby authorise any respectable member of the faculty to act on their behalf.

Our correspondents shall all receive answers in the next LANCET, when will be published the third lecture of Baron DUPUYTREN, together with several interesting papers and letters, omitted this week from want of space.